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OF THE

SOCIETY OF TELEGRAPH ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

W. E. AYRTON, The Chairman.

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TABLE OF CONTENTS.

Vol. VIII.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. VIII. 1878. No. 25.

The Seventy-first Ordinary, and the Seventh Annual, General Meeting of the Society was held on Wednesday evening, December 11th, 1878, at the Institution of Civil Engineers, 25, Great George Street, Westminster—the President, Dr. C. W. SIEMENS, F.R.S., in the Chair.

The PRESIDENT explained that the first business of the Special General Meeting was the consideration of certain alterations in the rules of the Society; that in regard to the alteration of Rule 24th, the Council had decided that in some cases the addition of as much as five shillings to the subscription would not be required, as the postage of the Journal, &c., might, in the case of members residing in foreign countries near home, come to less. It was therefore proposed to add to the subscription merely as much as was found necessary to cover postage, the excess being in all cases limited to five shillings. The modified proposed change therefore stood—

"That the 24th Rule of the Society be amended by the insertion after the word 'annually,' in the 6th line, of the following words:—

'Every Foreign Member and every Member or Associate residing abroad, or absent for nine months in the year, shall, in addition to his annual subscription, pay a sum to be fixed by the Council, not exceeding five shillings per annum, to defray the expense of postage of the Journal and other publications.'

This, being seconded by Mr. Preece, was carried. It was next proposed and seconded *that the* 28th Rule of the Society be

amended by the insertion of the following words after the word "Pounds" in the 4th line of that rule, namely:—

"A Foreign Member or a Member residing abroad who has compounded by payment of the sum of Ten Pounds, shall, if he come to reside in England, either pay an additional composition of Eleven Pounds, or an annual subscription of One Guinea.

And that there should be added:—

"28*a*. A Foreign Member who has not compounded, shall, if he come to reside in England, pay the same annual subscription as a Home Member, viz., Two Guineas.

"28*b*. An Associate who has compounded by the payment of the sum of Ten Pounds, shall, if transferred to the class of Members, either pay an additional composition of Eleven Pounds, or an annual subscription of One Guinea, and, whether he has compounded or not, he shall pay as Entrance Fee the sum of One Guinea, that being the difference between the Entrance Fee of a Member and that of an Associate."

Mr. LANGDON proposed to amend the suggested Rule 28*b*, by inserting after the words "One Guinea," in the third line, the following: "and if elected after the 1st of January, 1877, shall, whether he has compounded or not, &c."

After some discussion, in which the Treasurer pointed out that, since the commencement of entrance fees dated from the 1st January, 1878, it would be fairer that this should be the date mentioned in Mr. Langdon's amendment.

Mr. LANGDON expressed his willingness to accept this, and the Rule 28*b* was carried in this modified form, together with 28*a* and the addition to 28.

It was then proposed to add—

"28*c*. All Members shall pay the annual subscription for the year in which they are elected, without reference to the period of the year at which their election takes place; but they shall be entitled to receive a copy of all numbers of the Journal and other publications of the Society which may have been issued during that year."

After much discussion, in which Professor Abel, Captain Stiffe, Mr. Laister, and other members took part, and the proposal of an amendment by Mr. Roberts, which was lost, the original proposal was seconded and carried, with a verbal alteration proposed by Mr. Laister, to the effect that to the words "all members," &c., should be added "hereafter elected."

It was then decided that Rule 28*c* should not come into force *until the beginning of* 1879.

Scrutineers for the ballot of the new officers and Council for the ensuing year were then appointed.

Mr. LAISTER disapproved of the mode in which the ballot form had been drawn up for that year. He, without wishing to express the slightest disapprobation of the names of the new officers proposed by the Council, thought it highly desirable that more names should be proposed than there were vacancies, otherwise he was afraid the members would either come to regard the ballot as a mere form, or else a spirit of opposition to the Council might be engendered.

Mr. PREECE agreed with Mr. Laister in regard to the advisability of the plan advocated by him—the plan, in fact, that the Council had previously followed—but he mentioned that it was attended with certain disadvantages, one, viz., that before a man's name can be entered on the ballot list his sanction had to be obtained, and then if not elected there was much heart-burning.

Professor ADAMS regarded the small attendance at the meeting as a proof of the objectionability of the system of drawing up the ballot list adopted this year.

The PRESIDENT was of opinion, that although the Council had on the present instance acted as they thought for the best, a mistake had been committed, and that it was desirable that a latitude of names should in future be given in the official proposed list.

It was then proposed that the following rule be adopted :—

"39*a*. Any loan necessary for the immediate purposes of the Society in meeting current expenses, or discharging any debt incurred for current expenses, may be raised, pursuant to a resolution of the Council, on security of such property or funds (including future subscriptions) of the Society as may be available."

Mr. ANDREW BELL spoke of the unfortunate circumstance that the general success of the Society should be marred by an absence of financial success. He noticed that the debt to the printers at the close of 1877 was no less than £679, while, on the other hand, under the assets there was an amount of unpaid subscriptions of as much as £506 16s. 0d., or about ten shillings and ninepence halfpenny for each Member, home or foreign, and Associate belonging to the Society. He regarded this as a very large debt, when it was remembered that no fewer than 456 of our number

were Associates. He would like to know what means had been taken of enforcing payment, as it was quite obvious the subscriptions did not come in properly. He was of opinion a limit of time should be allowed, and that Members and Associates who did not pay should be struck off the list. He asked whether under the head of the property of the Society was included the Library.

The PRESIDENT then explained that the Ronalds' Library was held in trust and could not be touched, but there were some few books belonging to the Society and the stock of back numbers of the Journal, not very large, but, nevertheless, of considerable value. When the Society were in possession (as they would be in a few minutes) of the annual report, they would better understand the financial position. A considerable sum of money had been lost by people joining the Society as Associates, and then being transferred to the class of Members without paying the difference of the composition or entrance fee; by non-resident Members compounding and then returning to live in England, but without paying the remainder of the composition due as resident Members, &c. All these difficulties, however, had been overcome by the new rules passed that evening. The most serious difficulty had been the large, and gradually increasing, debt to the printers. A Special Committee of the Council was therefore appointed some weeks past to investigate thoroughly the financial position of the Society, and among many other resolutions, which have been seen this evening in the new rules proposed and passed, they recommended that the printing bill should be discharged, estimates obtained from a number of other printers, and new and more economical arrangements, both in large and in small matters, made for the future. A certain number of Members of the Society had already come forward and offered to lend, in sums of £100 or less, a total of not less than £700, the minimum sum necessary to discharge our debts, which being done, they would then be in a position to accept the tender for printing which seemed the best for the interests of the Society. The proposed Rule 39*a*, as well as the following, 39*b*, were then carried:—

"39*b*. Any loan for purposes of the Society, other than those mentioned in the foregoing Rule 39*a*, may be raised pursuant to a resolution of the Council, *confirmed by a resolution* of a General Meeting, on security of such property *or funds (including future subscriptions)* of the Society as may be available."

The Meeting then became an ordinary General Meeting, and the Secretary read the subjoined Report of the Council. Afterwards the Scrutineers, Messrs. J. Thewlis Johnson and J. Laister, announced that the whole list of the new Officers and Council, as proposed on the ballot list, had been elected, viz. :—

COUNCIL, 1879.

President.

LIEUT.-COLONEL J. U. BATEMAN-CHAMPAIN, R.E.

Past Presidents.

CHARLES WILLIAM SIEMENS, F.R S., D.C.L.
Professor ABEL, C.B., F.R.S.
C. V. WALKER, F.R.S.
LATIMER CLARK, C.E.
Sir WILLIAM THOMSON, F.R.S., LL.D.
FRANK IVES SCUDAMORE, C.B.

Vice-Presidents.

Professor G. C. FOSTER, F.R.S.
W. H. PREECE, M.I.C.E.
CARL SIEMENS, M.I.C.E.
Major C. WEBBER, R.E.

Members.

Professor W. G. ADAMS, F.R.S.
Captain ANDERSON, R.E., C.M.G.
W. A. ANDREWS.
WILLIAM T. ANSELL.
SIR CHARLES BRIGHT.
H. G. ERICHSEN.
Col. GLOVER, R.E.
CHARLES HOCKIN, M.A., C.E.
LOUIS LOEFFLER.
WILLOUGHBY SMITH.
C. E. SPAGNOLETTI, M.I.C.E.
HENRY WEAVER.

Associates.

CHARLES THOMAS FLEETWOOD.
ST. GEORGE LANE FOX.
Captain M. T. SALE, R.E.

OFFICERS.

Auditors.

J. WAGSTAFF BLUNDELL, 12, Delahay Street, Westminster, S.W.
FREDERICK C. DANVERS (India Office).

Hon. Treasurer.
EDWARD GRAVES.

Hon. Secretary.
Lieut.-Col. FRANK BOLTON.

On the motion of Mr. Langdon, the report was unanimously received, and a vote of thanks was passed to the President and

Council of the Institution of Civil Engineers for the privileges they continued generously to afford to this Society by granting the use of their hall for the meetings.

The following candidates were then announced as elected :—

Members.

Biggs, C. H. W.	Keith, C. E. George.

Associates.

Carew, W. R. H.	Irish, W. E., Sergt. R.E.
Collins, Ebenezer.	Knight, H. P., Lieut., R.E.
Crofton, A.I.C.E. Edward.	Lehmann, Charles.
Cuff, John C.	Rippon, J.
Davis, Joseph.	Scaife, Jno.
Davy, E. Fitzgerald, Commander, R.N.	Shield, Clifton.
George, Frederick.	Smith, Frederick.
Greig, A., Sergt., R.E.	Vere de Vere, Arthur.
	Weston, Robt. Wm. Gibbs.

REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING OF THE SOCIETY.

DECEMBER 11, 1878.

In making their Annual Report, the Council are glad to be enabled to state that the additions to the ranks of the Society during the year afford satisfactory evidence of the estimation in which it continues to be held, by those who, either directly or indirectly, are connected with the science and the art which it is its chief object to promote.

The number of new members of each class, including those candidates to be balloted for this evening, is as follows :—

Foreign members	23
Members	39
Associates	65
Students	1
	128

Four Associates have been transferred to the class of Members,

and the general list will, if this evening's candidates be elected, stand as follows:—

Honorary members...	5
Foreign members	172
Members	347
Associates	515
Students	13
Total... ...	1,052

It is to be regretted that a larger number of those young men who are preparing for the telegraph service, do not avail themselves of the great advantages offered by the Society to the class of students to whom practically the same privileges are accorded as to Members and Associates, at an almost nominal subscription.

One of the chief aims which the Society has had in view, has been the dissemination of an educational spirit amongst those who belong to its ranks; and the great object of the papers which are read at its meetings, and the discussions that result therefrom, is to spread amongst the inexperienced the knowledge and practice gained by the experienced.

The Society continues to enjoy the active assistance of those Members abroad who hold the office of Local Honorary Secretary and Treasurer, both in the way of collecting subscriptions and obtaining eligible candidates for election, and as constituting a convenient channel of communication between the Head-quarters of the Society and its Foreign Members. The following changes and additions in respect of the Local Honorary Secretaries have taken place during the year:—

	Appointed Local Honorary Secretary and Treasurer for
Mr. James Allen, Telegraph Superintendent of the Great Southern Railway of Buenos-Ayres	Argentine Republic.
Mr. Charles Burton, late Hon. Secretary and Treasurer for the Argentine Republic	Republic of Bolivia.
Mr. J. R. Preece, of the Indo-European Goverment Telegraph	Persia.
Mr. James Sivewright, Superintendent of the Government Telegraphs at the Cape	The Cape and South Africa.
Professor J. Perry, B.E., of the Imperial College of Engineering, Tokio	Japan.
Mr. Andrew Jamieson, Superintendent of the Eastern Telegraph Company *at Malta*	Malta and the Mediterranean.

Your Council look forward very sanguinely to the exertions of their Foreign Secretaries to add to the numbers of the Society, for it is those who are compelled by their vocations to reside abroad, away from the chief source of knowledge, who derive the greatest advantage from the dissemination of that information, and the circulation of those scientific facts which our journal of proceedings is intended to convey.

At the Ordinary General Meetings, which, by the continued liberality of the Institution of Civil Engineers, are still held in their large theatre, the following Papers and Communications have been read, and their titles alone afford sufficient testimony of the wide range of subjects which fall within the scope of the Telegraph Engineers' pursuits and studies, for they embrace not only questions having the most practical bearing upon Telegraphy, but others treating of the more profound details of Electrical Science :—

SUBJECT.	AUTHOR.
The American Telegraph System	W. H. Preece, V.P.
De La Rue's Chloride of Silver Battery ...	Latimer Clark, V.P.
Byrne's Pneumatic Battery	W. H. Preece, V.P.
The Logograph	W. H. Barlow, F.R.S., V.P.I.C.E
The Phonograph...	W. H. Preece, V.P.
Insulators for Aerial Telegraph Lines ...	John Gavey, Member.
The Law of International Telegraph Traffic	C. L. Madsen, Loc. Hon. Secretary, Denmark.
The Unit of the Birmingham Wire Gauge ...	C. V. Walker, F.R.S., V.P.
Sound in relation to the Telephone	Dr. Clarence Blake.
The Telephone Harp	Frederic L. Gower.
The Telephone	Lieut. Savage, R.E.
The Connection between Sound and Electricity	W. H. Preece, V.P.
Cable Grappling and Cable Lifting	A. Jamieson, Loc. Hon. Secretary, Malta.
Grapnels for raising Submarine Cables in Deep Water	Francis Lambert, Member.
Multiple and other Telegraphs at the Paris Exhibition, including Elishu Gray's Harmonic Telegraph	Major Webber, R.E.

Among the more important original Communications which have been received for publication in the Journal, may be mentioned the following :—

On Gaston Plante's New Electrical Machine. By Alfred Niaudet, Foreign Member.

Note on Electrolytic Polarisation. By Professors John Perry and W. E. Ayrton, Members.
The Resistance of Galvanometer Coils. By Professors W. E. Ayrton and John Perry, Members.
On Electro Magnets. By Oliver Heaviside, Associate.

The Council earnestly hope that Members of all classes will continue to communicate to the Society any information which may appear to them worthy of being either brought before the Society at the General Meetings or published in the Journal.

Some years past, one of your Members, Mr. J. Fahie, of the Indo-European Government Telegraphs, most liberally offered to contribute to the Society the sum of £100, for the purpose of providing an Annual Premium, to be awarded to the author of the best original Communication read before the Society each Session. At that time the Council did not feel themselves justified in availing themselves of Mr. Fahie's liberality, until the whole subject of granting Premiums could be placed on a permanent footing, and on a scale worthy of the Society. Mr. Fahie has recently renewed his kind offer, and the Council feeling that they would no longer be justified in declining it, have intimated to him their readiness to accept it, provided that the form in which, and the time at which the Premium shall be awarded, be left to the decision of the Council for the time being. By a telegram received last evening, we are informed that Mr. Fahie accepts these conditions, and that the money will be sent on the 1st of January.

Hitherto, the task of editing the Journal has devolved upon the Honorary Secretary and the Secretary, but with the increasing number of members, the correspondence and other secretarial duties have been also so largely augmented, that it has been considered desirable to create a new office—the "Chairmanship of the Editorial Committee." The post has been offered to, and your Council are glad to announce accepted by, Professor W. E. Ayrton, whose name is a sufficient guarantee that the duty of editing the Journal will be ably performed, and that it will maintain the high character it has already acquired as a work of reference for the Telegraph Engineer and the Electrician.

As the reception of the Journal is the most important privilege possessed by the foreign and *non-resident* Members and Associates,

it is thought advisable that exertions should, for the future, be made to publish the Journals at more frequent intervals, and that the part containing any particular paper should appear as soon as possible after the reading of that paper. To facilitate this, the contributors of papers are requested to send their communications in a *complete form* as early as possible *before* the evening on which they are read.

The Council are enabled to report that 91 pages of the Ronalds' Catalogue are now in type, and that arrangements are being made for further portions being set up forthwith. It would obviously be desirable, even if only on the score of economy, that the whole should be set up and printed at once, but, unfortunately, the number of subscriptions received up to the present time will not, it is to be feared, permit of this being done. The Council cannot refrain from expressing their disappointment that such should be the case, and they can only explain the circumstance by the supposition that the great value which the catalogue will possess as a work of reference is not thoroughly understood by many of the Members. They therefore desire to refer those Members and Associates who have not yet sent in their names as subscribers, to the very interesting description of the work given in the annexed paper, read before the Library Association of the United Kingdom, by Mr. A. J. Frost, the Acting Librarian of the Society, and to whom the preparation of the Catalogue for the press was confided.

With the completion of the Catalogue, the library of the Society will necessarily become of much greater practical value to the Members than has hitherto been the case; but in order that it may maintain that value, it will be necessary that it should be supplemented continually by the addition of all modern works on electrical subjects. The funds of the Society will scarcely allow of this being done to any great extent, and the Council would therefore urgently request that all members who contribute to the literature of that science which it is our aim to advance, will kindly present to the library of the Society a copy of their own works, as well as of any others of which they may possess duplicates.

In response to the suggestion urged by more than one influen-

tial member during the discussion upon Mr. C. V. Walker's paper on the Unit of the Birmingham Wire Gauge, your Council have appointed a committee to enquire into and report upon the several wire gauges in use, with the view to obtaining the adoption of a uniform gauge. The committee comprises gentlemen whose names are a guarantee that the subject referred to them will be exhaustively considered and examined.

At the request of the Meteorological Society, the Council have appointed two of its members, viz., Mr. Latimer Clark and Mr. Preece, to represent them at a conference consisting of members of that and other kindred societies, for the purpose of endeavouring to draw up a set of rules in respect to the use of lightning conductors, and at the first meeting of this conference, a third member of our Society, Professor W. E. Ayrton, was requested to join.

The meeting of the International Telegraphic Conference, which, in consequence of the disturbed state of European affairs in the East, was postponed this year, is now fixed to take place in London this next summer, 1879, and the Council feel assured that the members of the Society will look forward with pleasure to the opportunity which will thus be afforded them of welcoming so many of their most distinguished foreign *confrères*.

The annual Conversazione of the Society will therefore again be postponed, and the Council are gratified at being enabled to report that already a sum exceeding £500 has been subscribed to the special fund formed for the purpose of entertaining the members of the conference in a manner worthy of them and of the Society of Telegraph Engineers. Further subscriptions will be gladly received.

It is hoped, in connection with the Conversazione, to have the largest display of telegraphic apparatus that has ever been brought together in England, and every member of the Society is therefore personally invited to do his utmost in aiding the Council in carrying out this purpose.

The Council regret that they cannot close their report without again calling attention to the large amount of annual subscriptions which remain unpaid. This doubtless arises in many cases from mere thoughtlessness *on the part of* members; but it must

be remembered that with a Society like ours, where much is expected to be done for a comparatively small subscription, punctuality and regularity on the part of the members in paying that subscription is absolutely essential in order to enable the Council to carry on the work which the Society has in view.

A very strenuous effort has been made by the Council to place the financial position of the Society on a sound and solvent basis. A special committee has recently been appointed to carry out this purpose, and the result has been not only to effect considerable economy in the cost of management and publication, but to relieve the Society from a very heavy liability. It was found that the Society were indebted to their printers for no less a sum than £800; but, of this amount a sufficient sum having been advanced by some members, the Council will be able to free the Society from this pressing liability, and will thereby be in a position to make new arrangements in respect to the heavy item of printing, which will conduce to a great reduction in the annual expenditure, and the Society will commence the new year with a certain knowledge that their expenditure will be well within their income, provided that its members pay their subscriptions with punctuality.

The Council have deemed it desirable to submit to the members certain additions to, and alterations in, the rules of the Society, of which full particulars have been duly circulated among the members.

THE RONALDS' LIBRARY AND CATALOGUE.

By Alfred J. Frost,

Acting Librarian of the Society of Telegraph Engineers.

At the Conference of Librarians held in London last year, the writer briefly called attention to the fact that the Society of Telegraph Engineers was preparing for publication a complete catalogue of the literature relating to electricity and magnetism, and the cognate sciences. The catalogue referred to (which has been edited by the writer) was compiled by the late Sir Francis Ronalds, F.R.S., who devoted the greater part of a long life to its

completion, and to the formation of a valuable library bearing his name, now in the possession of the Society of Telegraph Engineers.

Sir Francis Ronalds, who became one of the earliest members of the Society, died in 1873, bequeathing the library to his brother-in-law, Samuel Carter, Esq.; and this gentleman, in accordance with the wishes of the testator that the library should be made available for students of electricity, handed the same over to the Society upon certain conditions, one being that the Society should bear the cost of printing the catalogue, which it had been the labour of its author's life to complete. The Society, although a very young one, willingly undertook the charge, and has spared no expense in making it worthy of its author, and of its importance in relation to science and bibliography.

The catalogue contains upwards of 12,000 entries, and is believed to contain a record of nearly all the important books and papers bearing on the subject, published in any language, up to within a short time of its author's death.

In the compilation of the catalogue, Sir Francis Ronalds adopted what is now known as the card system, thus using a separate slip for each entry. I am not aware who introduced this system, or the date of its introduction, but inasmuch as the catalogue referred to was commenced probably as early as 1820, I think Sir Francis Ronalds may be numbered amongst the earliest to see the advantages which that system bears over many others, especially in the compilation of a catalogue where many of the books are not seen, but have their titles, &c., copied from other sources. Every slip in the catalogue shows upon its face the whole history of the record. For example, in many instances it is found that the first notice of a work was obtained from some author's reference; later on—perhaps years—some further information is obtained of the work in, probably, an old bookseller's catalogue, the slip is looked up and the further particulars are added, the source of the information being given in every case; later on again, perhaps a further reference to the work is found in the catalogue of some public library, in which case any further information thus obtained is added to the slip, and lastly the book itself finds its way into the library, when all doubt as to the accuracy of the entry

is at an end, as the actual title is then examined with the slip, and any particulars which have been omitted are inserted. Another advantage of using a slip for each entry is the facility given for making notes. Had the titles been entered into a book it would have been very difficult, and almost impossible, to have added so much from time to time, as it was found necessary to do, in the work now referred to.

It was considered desirable to preserve as many of the author's notes and references as possible, and these notes will form quite a special feature in the Catalogue, besides which there will be found after most of the important names a reference to the date and place of birth, and when possible the date and place of death of the author. Although this information is not usually given in printed catalogues, it was thought that it would not detract from its value if inserted, but would, on the contrary, enhance its importance, and render the work more valuable as a book of reference.

The publishing committee of the Society gave considerable attention to the type most suitable, and it was decided to adopt large Clarendon type for the authors' names; all notes, references, &c., otherwise than the actual titles, being printed in italics; special prominence has been given to dates. The books actually in the library are designated with a dagger.

Although the Catalogue professes to relate simply to Electricity and Magnetism and their applications, it will be found a most valuable record of scientific books generally, inasmuch as almost all books treating of Natural Philosophy, and Physical Science, contain something electrical, and it was of course necessary to include all such books in a work of this nature.

The Catalogue will be published at a price much less than its cost, and it is proposed to issue to subscribers a separate Librarian Edition, printed on one side of the paper only, the price of which will be 20s., the price of the Catalogue in the ordinary form being fixed at 16s.

The library formed by Sir Francis Ronalds, above referred to, contains about 10,000 works, a large number of which consist of electrical papers cut from Philosophical Transactions of the Royal Society, and from the transactions and proceedings of other English

and foreign public societies, and from scientific periodicals, &c. The collection is a very complete one, particularly in Italian, French, and German, and may almost be said to be unique.

Sir Francis Ronalds has been long known to the scientific world as the author of a small and now scarce book, the first ever published on the subject of the Electric Telegraph.*

This book describes a system of Electric Telegraphy which its author invented and worked as early as 1816. The invention was a perfectly practicable one, and has gained for its author, from more than one learned authority, the title of "The father of Telegraphy." The invention, however, was produced some thirty years before the world was prepared for it, as is shown by the reply of the Secretary of the Admiralty in dismissing an application which had been submitted to him. It was as follows:—

"Mr. Barrow presents his complements to Mr. Ronalds, and acquaints him, with reference to his note of 3rd instant, that telegraphs of any kind are wholly unnecessary, and that no other than the one now in use will be adopted.—5th August, 1816." †

The result was that the telegraph remained nothing more than a successful experiment. Sir Francis Ronalds was, from his scientific attainments, and from his knowledge of foreign languages, well fitted for the task he set himself, and it is very much to be regretted that he did not live to see his work issued from the press. He received the honour of knighthood about three years before his death, in recognition of his early contributions to electrical science.

In conclusion, it may be added that, owing to the fact that most of the books and pamphlets are in an unbound condition, the library has not yet been opened for reference. The binding will, however, be quickly proceeded with, when the library will, under certain conditions, be opened to students and those interested in the subject to which the collection refers.

At the conclusion of this Meeting, two of M. D'Arlincourt's fac-simile telegraph instruments were shown in operation.

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* Descriptions of an Electrical Telegraph, and of some other Electrical Apparatus. By Francis Ronalds. 8vo. London, 1823.

† Journal Society *Telegraph Engineers*, Vol. IV., p. 5, 1875.

The Seventy-second Ordinary General Meeting of the Society was held on Wednesday Evening, January 22nd, 1879, at the Institution of Civil Engineers, 25, Great George Street, Westminster, the Past President, Dr. C. W. SIEMENS, F.R.S., in the Chair.

The minutes of the last meeting having been read and approved,

The CHAIRMAN rose and said: Gentlemen—It is my pleasant duty to present to the Members of the Society my successor in office, Colonel Bateman Champain (applause). He is so well known to you all, as well as to the whole telegraph world, that no recommendations on my part in his favour are necessary in this introduction.

Lieut.-Colonel BATEMAN CHAMPAIN then took the chair, and after the names of new candidates for election had been announced by the Secretary, the new President proceeded to read the following address:—

INAUGURAL ADDRESS.

By Lieut.-Colonel J. U. BATEMAN-CHAMPAIN, R.E., President, 1879.

Gentlemen,

Permit me to express my deep sense of the great honour which has been conferred upon me by my election to hold the office of President for the year 1879, and to assure you that it was with the most unfeigned diffidence and reluctance that I accepted a position which has always until now been filled by the most illustrious members of our Society. At a later stage of my remarks I hope to give a reason (although perhaps an insufficient one) for my presumption.

The Annual Report of the Council for the past year shows that the progress of our Society is steady and satisfactory. We now number in our ranks more than one thousand members, associates and students, and we may reasonably hope that every year will add our strength; the science, indeed, whose votaries we profess to is making such amazing strides, and exciting in all lands such

a wide-spread interest, that it is fair to anticipate a larger increase in our Society, and a still greater prosperity.

It will be seen that the important question of finance has been recently attracting the special attention of our Council. The Society is now, in consequence of the loan generously offered by a few members, well established on a firm footing, but we are as yet a comparatively juvenile institution, and have no accumulation of funds in reserve, so that it will be necessary for some time to pay the closest attention to economy in small as well as in great matters. Any special outlay, such, for example, as that consequent on the bequest in trust of the Ronalds' Library, rather strains our resources; but it is satisfactory to know that the economical measures proposed by the Sub-Committee of the Council will greatly reduce our annual expenditure, and enable us to commence the year 1879 with an unclouded prospect; indeed, in the heavy item of printing and publishing a very material saving has already been effected by the labours of the recently-appointed "Chairman of the Editing Committee."

As one of the chief privileges possessed by our members residing abroad is the regular reception of our Journal, by a perusal of which they can keep themselves *au courant* with the progress of telegraphic science in England, it is extremely desirable that the Journal should appear more frequently than hitherto, and that the numbers containing any particular paper should be in the hands of all members very shortly after that paper has been made public at the meeting of the Society. To enable this, however, to be accomplished, it is necessary that the writers of communications should forward them to the Secretary in a complete form, both as regards letterpress and figures, several days before they are to be read, a rule which it is hoped will in future be adopted by the authors.

It is unnecessary for me to attempt to point out how numerous and varied are the subjects which legitimately engage the attention of a Society such as this.

The application of electricity to the every-day requirements of civilised life is being extended with marvellous rapidity, and

scarcely a day now passes without producing fresh evidence of the energy and perseverance of the practical electrician.

The valuable and interesting papers which are read at our meetings treat more or less exhaustively of all matters directly or remotely connected with electricity and magnetism, and the published transactions of the Society afford ample evidence of the wide scope of the scientific efforts of our Institution.

In the following short and imperfect retrospect of the main achievements of the year just closed, I shall confine myself to the broad facts, and avoid as far as possible any expression of my individual views.

Among the most striking events connected with electricity, the great agitation which has taken place during the past year on the electric light stands prominently forward.

It would be out of the question for me to pretend to recapitulate the many more or less important and successful contrivances which have been recently devised with the object of generally utilising (if I may employ the expression) the two grand discoveries of Sir Humphrey Davy and Faraday. As regards the lighting of streets by electricity, it is perhaps a little remarkable that the idea has been taken up with greater alacrity on the Continent than in England. London has not led the way as she used to do, and we cannot boast as Byron did half a century back—

> "The line of lights, too, up to Charing Cross,
> Pall Mall, and so forth, have a coruscation
> Like gold, as in comparison to dross,
> Matched with the Continent's illumination,
> Whose cities night by no means deigns to gloss:
> The French are not yet a lamp-lighting nation."

On the contrary, the experiments first tried on an extended scale in the streets and squares of Paris, Madrid, and other Continental cities, appear to have set an example to the Londoners, and trials of numerous systems are at last being made on the Thames Embankment, the Holborn Viaduct, the Billingsgate Market, the Mansion House, and elsewhere.

Mr. Louis Schwendler, one of the members of our Council, who has just left for India, has been carrying on some laborious tests with the view of selecting the best plan of adapting the electric

light for large Indian railway stations. For this, adopting the opinion of Dr. Siemens and other eminent authorities, he advocates the use of large lights few in number, and believes that, by careful attention to diffusion and reflection, the result will be preferable to that obtained by subdivision.

Although the experiments now being conducted may not conclusively settle the question of the value of electricity as a means of illumination, they cannot fail to be useful and instructive, and will undoubtedly lead to endless modifications and improvements. The past year will henceforth be remembered as that in which the battle of electricity *versus* gas commenced.

It is, I suppose, certain that, for many purposes where gas has hitherto been employed, it may with advantage be superseded by its more dazzling rival, more particularly in the case of large areas. The difficulties as regards the subdivision of the electric light, the improvement of its steadiness, and the great degree of care required by the apparatus, have not yet been, and may never be, entirely overcome. But we know from the numerous patents taken out, and from the countless letters and papers published on the subject, that some of the most inventive and practical men of the age are concentrating their energies on the endeavour to solve these problems. To quote from an article which lately appeared in the *Times*, "the development of inventive genius in this direction has been very marked of late; the stimulus given to it has been very great; and the records of the Patent Office disclose the fact that the numbers of patents for improvements in electric lighting has enormously increased in 1878 over previous years. This increase has been greater during the past few months than it was in the earlier part of the year." The article goes on to state that up to the end of November last no less than 78 such patents had been applied for, while during the whole of the previous year the number had been but 13.

It is not impossible that, by the judicious employment of large lights, our crowded river may be illuminated by the electric rays, so that navigation may be carried on almost as easily by night as by day. The marvellous superiority of the electric light over every other in piercing the thickest fogs and blackest darkness

must surely lead to its more general adoption on board ship, and tend perhaps more effectually than anything else to diminish the risks of disastrous collisions.

It has been clearly shown by our late president that, however much electricity may be utilised for illumination, there will still remain a practically unlimited field for the development of gas as a heating agent.

Mr. Graham Bell's telephone continues to be the subject of study and experiment. Difficulties arising from induction between wire and wire, and the rather feeble volumes of sounds produced, appear as yet to interfere with the practical employment of the instrument on long circuits.

That wonderful invention the phonograph, by which Mr. Edison has immortalised his name, cannot be omitted from the the list of remarkable inventions of the past year, as, although it is scarcely a telegraphic, and is not an electrical instrument, it may be pronounced nearly akin to the telephone. Should we be able to actuate a phonograph by a telephone it may some day be possible to transmit articulate sounds to a distant station, record them on tinfoil, and deliver them by messenger to be reproduced by aid of a second phonograph. Speaking machines have been attempted by many laborious and patient inventors, but have generally resulted in complicated failures; and the phonograph is as remarkable for its complete reproduction of the human voice as for its extreme simplicity.

The discoveries of Professor Hughes regarding the effect on the electric current of sound vibrations, through the agency of what may be termed the resistance of numerous sensitive contacts in a circuit, by which minute sounds are reproduced and magnified at a distance in a telephone, by the instrument called the microphone; as also the invention of the carbon telephone by Mr. Edison, and the innumerable combinations and modifications of these instruments which have sprung from their discovery, form altogether a new branch not only of electro-telegraphy, but indeed of electro-physics.

The micro-tasimeter of Mr. Edison, by which the most minute *change in* temperature can be detected, will no doubt prove a

valuable instrument in the laboratory and in physical research, and is another instance of the useful application of electricity to other branches of scientific investigation.

Mr. Willoughby Smith's discovery of the effect of light on the resistance of selenium has been followed by Mr Sabine's discovery of the effect of light on selenium in producing a voltaic current.

Byrne's pneumatic battery, in which the introduction of a current of air increases in a remarkable degree the electrical force, is a complete novelty in electro-motors, and may originate an entirely new class of batteries.

It would be unpardonable to omit all mention of the remarkable collection of telegraphic apparatus exhibited at the Paris Exhibition of 1878. Major Webber, to whom this society owes so much, held a distinguished position in connection with the award of prizes in this particular class; he had special opportunities, therefore, of thoroughly examining the various electrical inventions displayed, and his interesting paper on some of the most striking of these instruments, which was read in this hall a few weeks back, is fresh in our recollection.

Turning now to submarine enterprise, and looking back on the past year in connection with this important subject, I would first note that the final abandonment of two of the Atlantic cables may be looked on as furnishing some information as to the "life" of that type of cable in the depths of the Atlantic. Whatever may have been the immediate cause of their failure, there can be no doubt that in the course of 12 years they have, in consequence of the decay of the covering which constitutes their mechanical strength, become too weak to admit of being lifted for repair; and the attention of engineers may advantageously be directed to the more efficient protection of the materials of this covering, so as to render it possible to raise these costly cables after long periods of submergence.

The durability of the strength-giving sheathing is at present quite disproportionate to the practically imperishable character of the insulating material, although this latter is subject to mechanical injury in various ways, or may contain a latent fault when laid.

The *destructive action of marine* animals on the cables near

Singapore has led to the adoption by the Eastern Extension Telegraph Company of a core protected by a metallic taping, which will no doubt add to the security of cables liable to the attacks of the teredo and similar enemies.

The Eastern Company has laid during the past year about 630 miles of cable in the Levant, besides a cable 327 miles in length from Egypt to Cyprus, and the West India and Panama Company has laid one from Cuba to Jamaica, 150 nautical miles in length.

The manufacture of 2,550 miles of cable has been commenced at the Telegraph Construction Company's works, for the Eastern Extension Telegraph Company, in order to duplicate the line from Penang, *via* Singapore, Banjoewanji, Java, and Port Darwin.

A line from Aden to Zanzibar, Mozambique, Port Natal, and Algoa Bay, with a branch line from Zanzibar to the Mauritius, is, I believe, contemplated by the Eastern Telegraph Company, and will perhaps be commenced at an early date.

Some notion of the magnitude of the operations of private enterprise in relation to submarine telegraphy may be gathered from the fact that upwards of 20 large steamers, with an aggregate of nearly 30,000 tons, are constantly employed in different parts of the world for the sole purpose of maintaining and repairing the many cables which now lie in almost every sea.

With regard to the improvements in the speed of signalling, and amount of traffic-bearing capacity, we may note that on the direct United States cable, 2,420 nautical miles long, and also on the Madras-Penang line, a duplex balance has been successfully obtained by Messrs. Muirhead and Herbert Taylor, using Muirhead's artificial cables. One of the Anglo-American wires has been duplexed also by Mr. Stearns on a similar principle.

The appliances invented for increasing the speed of signalling are so numerous that I cannot pretend even to give a list of them. I may, however, just refer to the ingenious instrument of M. Baudot, for economising the time of a wire by sending many messages concurrently by means of what may be called the distributor system, first introduced by Meyer, as worthy of record on the year's list of novelties, although it was produced, I think, before January, *1878.*

Mr. Elisha Gray's harmonic telegraph, exhibited in this room at a recent meeting, also deserves special mention.

In damp climates, and especially on sea coasts, the insulation of land lines is a matter of great difficulty, and the fluid insulator of Messrs. Johnson and Phillips, in which a zone of oil is interposed to prevent surface conduction, promises to overcome this difficulty. We have some of these insulators in use on the Mekran coast, which have given very favourable results.

Now, gentlemen, although the improvement and perfection of the apparatus and machinery in use for the transmission of messages are undoubtedly objects of the first importance to us, yet I am of opinion that, as "Telegraph Engineers," we may with advantage direct attention now and then to the means by which that machinery is employed and controlled.

I propose, therefore, this evening to make a few observations on the practical administration of the telegraph system, on the efficiency of which the utility of one of the most marvellous of all modern inventions very largely depends.

I am aware that the subject may be considered somewhat commonplace by many here assembled, but I must humbly own that I am fully conscious of my inability to guide you among the higher mountain paths familiar to my distinguished predecessors,—I must plod in the valley or stay at home,—and I am sensible that I owe you some explanation of the reason which led me to accept the unmerited honour which has been conferred on me, and to address you as your President this evening.

It is simply this. This summer will, we hope, witness the assembly in London of one of those international telegraph conferences which are held from time to time to revise, and, where necessary, to modify, the legislation accepted by the adhering States. It has been my duty to attend one or two of such conferences, and, consequently, I have gained some knowledge of the several steps which have led to the adoption of the laws now in force. I may add that it is my good fortune to be on intimate and friendly terms with many of the foreign delegates whom we hope to welcome in London, and I flattered myself that I might as your President this *session be able to* render some small service to the

Society. It struck me, too, that an explanation of the past history and efforts of these conferences might not be inopportune at the opening of the year 1879.

In the early stages of the development of the electric telegraph, when the wires were gradually creeping from city to city, and when the telegraphs of each country were, so to speak, self-contained, each administration made its own rules and cared nothing for its neighbour. It was not until nearly the middle of this century that the lines of one country began to cross the frontiers and to hook on to those of the adjoining States, and international telegraphy became an established fact.

The necessity of well-defined laws for the regulation of the international traffic was immediately felt, and from comparatively small beginnings, to which I shall devote a few words, the present complete and elaborate code has been drawn up, and is now accepted over nearly the whole civilised world. The International Convention does not, of course, lay down laws for the internal traffic of the several States; one country may choose to tax its local messages by the word or even letter; a neighbouring country may, if it pleases, tax on the 20-word system and give the address free; but international messages, that is to say, messages crossing the frontiers, are all taxed and generally treated in strict accordance with Convention laws. Just as in railway administrations, one State may select for its internal traffic a broad gauge, another a narrow, and a third the metre gauge; but directly an international system of railways comes into play, the States interested must obviously agree among themselves on a uniform standard and on uniform working rules. Nowadays, international conferences to legislate on railway, postal, metric, and other subjects are frequent, but telegraphy was the first to be so treated, and since the date of the earliest meeting in Paris, 26 years ago, periodical assemblies have been held with considerable regularity.

Like almost everything else, these conferences had small beginnings. The original one was attended by representatives of the following Governments only: Belgium, France, Prussia, Austria, Bavaria, Saxony, Hanover, Wurtemburg, and Holland. State after State then began by degrees to join, until at the last

meeting at St. Petersburg all the nations of Europe, besides three or four extra European countries, were parties to the International Telegraph Convention. From 1852 to 1858 these conferences were, comparatively speaking, rather numerous; partly because different arrangements were entered into between different groups of administrations, and partly because in those early stages of telegraph administration want of experience necessitated frequent revision and remodelling.

The dates of the eleven chief conventions which have been concluded are as follows:—

Paris	4th October, 1852.
Berlin	29th June, 1855.
Paris	29th December, 1855.
Brussels	30th June, 1858.
Berne	1st September, 1858.
Friedrichshafen	26th October, 1858.
Paris	18th May, 1865.
Vienna	21st July, 1868.
Berne (supplementary),	2nd October, 1871.
Rome	14th January, 1872.
St. Petersburg	7th/19th July, 1875.

The rule at present is to meet once in about three years; a conference, therefore, would in due course have assembled last year, as anticipated in the opening address of our late president; but from the disturbed condition of Europe and other causes it was postponed until the summer of 1879.

At the earlier conferences each State was represented by a diplomatic official of rank, but latterly the delegates have usually been officers of high standing in the telegraph administrations of the several countries, and these delegates have powers to agree to the generally-approved alterations, subject to eventual ratification by their respective Governments. India accepted in a general way the Convention rules almost immediately after the completion, in 1864, of the Indo-European line of telegraph, which connected the Indian system with that of Europe, and she joined formally at Vienna in 1868, *when Colonels* Goldsmid and Glover were

India's delegates at the conference. I may observe that, although it has been the custom for each State to send more than one representative official, a single vote only is granted to each distinct Government.

In 1868 the telegraphs of this country were not, as now, worked by the Post Office, and, consequently, Great Britain was not represented at the Vienna Conference. The first cable between this country and the Continent was laid, I may remind you, in 1851, and the British companies soon found themselves obliged to treat their foreign traffic in accordance with the international regulations. England joined the Convention after the purchase of the inland wires from the companies, and Messrs. Chambré and Fischer, of the Post Office Telegraphs, were for the first time deputed to represent this country at the Rome Conference of 1872.

One vote was given to Great Britain and a separate one to India, in consequence of the independent character of the administrations of the two Governments and the different interests they might have in view.

It will be obviously impossible for me, within the limits assigned to a presidential address, to describe to you fully the legislation affecting international traffic and tariffs established at the different conferences; but by confining my attempts to a brief outline of the original Convention of Paris, with a sketch of two or three of the principal modifications subsequently introduced into it, and the chief provision since made to keep pace with the requirements of the times, I hope to furnish you with some materials for forming an opinion on the value of the labour expended in this particular branch of our profession. And here I would draw your attention to what appears a rather remarkable fact, that although upwards of a quarter of a century has elapsed since the first Convention was drawn up between the representatives of a few States at Paris, its enactments, so far as they go, have virtually formed the groundwork of all our subsequent legislation: a circumstance which bears emphatic testimony to the care and completeness with which they were thought out and prepared.

The main provisions of the original Convention, signed at Paris *in 1852,* were the recognition of the right of all persons to use the

international lines on prepayment of their messages; the stipulations that messages from the public should be written in an intelligible manner; that the languages employed should be French, German, or English; and that senders should furnish proof of identity when required, and that Government messages only might be sent in cipher. The contracting States engaged that a sufficient number of wires should be set apart for international traffic; that the best instruments should be made use of; that secrecy should be maintained; and that the charge for any message lost, mutilated, or seriously delayed, should be refunded. This was the limit of administrative responsibility, which has not been since extended. It was arranged that all messages should be classified and transmitted in the following order:

1, Government messages;
2, Service messages between telegraph administrations;
3, Messages from the public;

that the telegrams of each category should be signalled in the order of their deposit by the senders, or of their arrival at the intermediate stations; and that the signalling of any message once commenced should not be interrupted to give place to a despatch of superior rank, except in cases of absolute emergency. It was provided that messages stopped in consequence of interruptions of the lines should be forwarded by post; that all international offices should be open during specified hours; and that any sender should be at liberty to prepay a reply or an acknowledgment of receipt, or to have his message repeated *en route*, to ensure greater accuracy. The Governments accorded the power to other States to accede to the Convention on demand; and each reserved to itself the right to suspend the service on its lines, either altogether or for certain classes of messages, on condition of giving immediate notice to the other contracting Governments.

The unit of charge was for a standard message of 20 words, with double rates at night; and each contracting State was divided into a number of zones, with a separate charge for each zone, according to its distance from the frontier.

Considerable *modifications have* since been made in the length

of the standard message and in the tariffs for each country. It is, as you are aware, necessary, for purposes of record and identification, that each message should be accompanied by a preamble, giving its official number, date, place of origin, and other particulars. As the preamble is equal to 6 or 7 ordinary words, it appeared natural to those who drew up the first tariffs that a standard message should be of such a length that its cost should cover the transmission of the preamble. In the first Paris Convention, a single rate was accordingly made to apply to a standard message not exceeding 20 words in length; two rates were demanded for a message containing from 21 to 50 words, and three for a message containing from 51 to 100 words. During the next few years the standard was altered to 25 words, and then to 15 words, with a limited free address. In the Berne Convention of 1858, the latter was discontinued, and the standard was brought back to the original number of words, with half-rate for each extra 10 words, and this regulation has remained in force in Europe ever since, that is to say, for about 20 years.

But although suitable for lines in Europe, where the distances are comparatively short, and the tariff moderate, it soon became evident that a 20-word standard was ill adapted to the long lines connecting Europe with India and other distant countries. These extra European lines are very costly to construct and to maintain; their traffic consists, moreover, almost entirely of messages passing between their extreme points, the business of the intermediate stations being but insignificant. The tariff on lines of this description is therefore necessarily high, and, where this is the case, merchants, who are the most important customers, soon find that they can express the bulk of their wants in messages of very much shorter length than 20 words; in fact, by arranging a number of selected words with different meanings attached to each, they are enabled to form codes by which they can convey all their ordinary enquiries, instructions, and replies in very few words indeed. To meet the demand for brief messages beyond Europe, the Vienna Convention of 1868 allowed the extra European administrations to introduce on their lines a special message of 10 words. The Rome Convention

of 1872 authorised the conversion of the special message into a standard message of 10 words, with subsequent gradation of charge per single word; and the St. Petersburg Convention of 1875 swept away altogether the restriction of the 10 word minimum, and introduced on the extra European lines a word tariff pure and simple. In fact, the European administrations recognised the word measure for extra European traffic, but retained the 20-word standard in Europe. The former is, however, now meeting with much general favour and support. It has quite lately been adopted by special agreement between Germany and Great Britain, and it is not impossible that at the London Conference it may be accepted as the normal international unit. The arguments in favour of a small standard of measurement are obvious, but naturally apply much more forcibly in the case of high than in the case of low tariffs.

It is not fair to insist on a sender paying for more work than he wishes to exact, and it is to the interest of all that the charge for a message should be as nearly as possible in proportion to the time and labour expended.

A letter tariff has been suggested, and this would, perhaps, in theory be a nearer approach to perfect accuracy than the word, but in practice it would be exceedingly troublesome, and the clerical labour entailed would be excessive.

Adverting to the tariffs, which, as I previously mentioned, were based in the original International Convention of 1852 on the zone principle, I would observe that, roughly speaking, the first zone of a country was about 50 miles in breadth, and the succeeding zones about 100 miles. The charge in 1852 was 2s. per zone for a standard message, and by successive reductions this rate was brought down to about 1s. 2d. in 1858.

In 1865 was concluded the seventh Convention on my list, generally spoken of as *the* Paris Convention, though it was, as I have shown, not the first or only one drawn up in the French capital. It acquired its repute from the length of its duration and its subsequent revisions at Vienna and Rome. On this occasion the zone system was abolished, and the principle of mean rates for each country was substituted. The tariffs were also very considerably reduced, as may be seen by taking, as an example, the 20-word

charge between London and Constantinople, which in 1854 was 19s., but only 9s. after the Paris Convention.

In the St. Petersburg Convention of 1875, the principle of mean rates was, at the instance of India, somewhat further extended, so that, in the case of messages exchanged with the East, a mean rate is now collected to cover the transit over any distance in Europe. This is another principle which seems to meet with growing approval, and before long we may have a mean telegraph rate of so much per word between the different States of Europe, resembling the mean rate for letters established at the Postal Union.

The question of how far the telegraph rates can be cheapened, and this costly means of communication placed within the means of a larger class of correspondents than at present, is one of the greatest interest. Mr. Edwin Chadwick, and many other gentlemen whose opinions deserve attentive consideration, think that the time has arrived when the ocean telegraphs should be purchased by the Government, and as long ago as February, 1872, a paper giving expression to these views and to the arguments n their support was circulated by the Society of Arts. An elaborate and able essay on the subject of tariffs, a translation of which appeared in the *Telegraphic Journal*, was written not long ago by M. Vinchent, one of the most distinguished of all authorities on international telegraphy, who has for many years represented Belgium at the Conferences.

It would be fallacious to assume that the conditions which govern telegraphic correspondence are precisely the same as those which affect the postal traffic. It certainly does not follow that because the charge for the transmission of a letter over long distances has been brought down to a penny or two, and the result notwithstanding has been beneficial to the revenue, therefore a reduction of the telegraph tariffs from shillings to pennies would likewise pay. Postal work can be conducted on what may be called wholesale principles; telegrams must be manipulated one by one. The real labour of a letter is performed by its writer, while in a telegram all is done by the departmental official. Roughly speaking, the carrying capacity of postal vans and mail steamers

is unlimited, while that of a wire is restricted to so many words a minute. While, therefore, I cannot suppose that we shall ever transmit our telegrams as cheaply as our letters, I still venture to foretell a gradual but steady diminution in the tariffs as at present framed. Every mechanical invention which increases the working speed of our wires must tend to remove one of the principal obstacles to the growth of cheaper telegraphy. For my part, I would even advocate the freer employment of reasonable codes: I mean those made up of words of medium length, easy to send, and not liable to error, which necessitates repetition. I am bound to say that some of the secret words now used, especially on long lines of telegraph, are the reverse of reasonable, and actually occupy the wire and worry the clerks far more than would the transmission of the full sentences for which these bewildering words are abbreviations. I should like to give you instances of some of the remarkable conglomerations which have come under my notice, but the time at our disposal would hardly admit of my spelling out any of the very long specimens.

A remarkable paper on the law of international telegraph traffic, by Mr. C. L. Madsen, our local honorary secretary for Denmark, was read before this Society in April last. The object of that paper is to show the existence of a fairly uniform mathematical relation between the commercial and the telegraphic traffic of the several States. The entire question is one which deserves close and careful study.

Attempts have been made at different times to induce the Governments of Europe to declare the submarine and land lines of the world neutral in time of war. The United States, I believe, addressed a circular note on the subject, about 7 or 8 years ago, to the different Cabinets, and at Rome, in 1872, Mr. Cyrus Field, as representing the telegraph interests of America, laid the matter before the Conference, with a view to the insertion of some protective clause in the International Convention. The delegates, while admitting the desirability, from a philanthropic point of view, of such neutrality, if practicable, considered the discussion of the question to be beyond their functions. They, however, inserted a note in the *proces verbaux*, drawing the attention of their Governments to

the application. Nine Governments replied: some commenting in a general way on the civilising tendency of the proposal, others referring to the replies they had previously sent to Washington. The result, as might perhaps have been anticipated, was that no decisive action has been taken in the matter.

I need not dwell at any very great length on the many other modifications which have been introduced into the Conventions of later years. They may be thus roughly summarised :—

The principal international offices must now be kept open throughout the twenty-four hours, and all extra night charges have been done away with.

A very large number of languages are now accepted as suitable for correspondence, it being however stipulated that every message when tendered be clearly written in the Roman character.

Permission is now freely accorded to all to employ cipher, or secret language.

The maximum length of a word which at first was no less than 7 syllables, is now 15 letters on the European, and 10 letters on the extra European systems.

The franc has been universally accepted as the monetary unit in which all international accounts have to be prepared.

Rules have been drawn up to improve and regulate semaphoric communication with vessels at sea.

A complete system has been devised for the uniform settlement of traffic accounts between the several administrations, and sundry permissive clauses have from time to time been inserted, allowing separate administrations to make among themselves special arrangements which do not affect the other contracting States. This covers the introduction on some systems of what are called "emergent" messages, at treble rates, having the advantage of priority of despatch, also, on the other hand, of a cheaper class of despatch written on telegraph cards. The States are also empowered, if they please, to introduce, for the use of the Press, a system of subscription for the employment of unoccupied wires at reduced rates during certain hours of the night.

Every Convention since that of Brussels has been translated by Mr. Alfred Brasher, Superintendent in the Indo-European Tele-

graph Department. The translations were first made for the use of the Indian Telegraph officials only, but for the last ten years the Secretary of State has authorised their publication, and they have been adopted by the Post Office, the English companies, and, in fact, by all the English-speaking telegraph administrations in the world, without, so far as I know, any impeachment of their accuracy.

The headquarters of the International Telegraph Administrations are at Berne, and the business has been for many years past conducted by M. Curchod, a gentleman of consummate tact and industry. His duties are by no means light, and I may say that the ability and courtesy with which they have been executed are gratefully recognised by every department concerned.

This central bureau receives and circulates notices of the opening of all new lines and stations, as well as of all interruptions and their rectification. To Berne are addressed all proposals for modification of rules or tariffs, which are then formulated and sent on to the different administrations interested. At the Berne office, too, are prepared new international tariffs and revised telegraph maps. It publishes a telegraphic journal, and the arduous task of arranging the materials for every fresh conference lies with the director, who invariably attends in a consultative capacity.

The expense of maintaining this central office at Berne is borne by the contracting States in different proportions, according to the importance of each administration.

Before the members of the conference disperse, they decide by vote where the next assembly shall take place. I am rather inclined to think that the conferences recur at too short an interval, and that a meeting once every five years would now be quite sufficient. Some twelve months before the date fixed for the new conference, the head of each administration is called on by the director of the central bureau to forward to Berne all proposals for change of regulation or tariff which it may be deemed desirable to discuss. The several proposals are carefully classified and circulated, so as to allow ample time for all to examine and criticise, and to make ready their arguments, whether favourable or adverse. At the appointed time, the delegates, armed with proper credentials from their respective Governments, present themselves at the

selected place, and are formally received by some State Minister, who welcomes the visitors, and then instals in the presidential chair the Director-General, or some other official holding high rank in the telegraph service of the country where the conference is held. At Vienna the delegates were received by Count de Beust, and at Rome by Signor Visconti Venosta, both Ministers of Foreign Affairs; at St. Petersburg by General Timatscheff, Minister of the Interior. The presidents at the three conferences were respectively M. Brunner de Wattenwyl, Signor D'Amico, and General von Luders, and it would be hard to say to which of these admirable officers should be awarded the palm for the complete success which attended the arrangements.

It is obvious that a very great deal depends on the personal qualifications of the president, in whom should be combined patience, firmness, and courtesy, with a thorough knowledge of all the requirements of the telegraph organisations. He should, moreover, possess an intimate acquaintance with the actual working of the international rules.

I should here state that the British companies, to whom telegraphy owes so vast a debt, were strongly represented at Berne, Rome, and St. Petersburg. They had no voting power, but were at liberty to express their views fully and freely, and I am sure they had no cause to complain of want of support or of inattention to their requirements.

The system usually followed at these conferences is to tell off the 30 or 40 delegates into committees. One committee occupies itself with the revision of the rules, a second with the arrangement of the tariffs, and a third with the task of editing the Convention as a whole. The ordinary duration of a conference is from one to two months. The members meet three or four times a week, each sitting lasting from about ten in the morning until five or six in the evening. The work of the committees occupies the by-days. Throughout the deliberations the French language only is employed.

These international telegraph conferences by no means consist of "all work and no play." At Rome and St. Petersburg every effort was made to entertain the visitors, and all the sights of these two *capitals* were thrown open to us. Banquets were given in our

honour at the royal tables; special nights set apart for us at the operas, and so on. In Italy we were carried off in a body to Naples, and fêted most hospitably. A special excavation was arranged for us at Pompeii, and some amazing antiques and bronzes were brought to the surface. If I did happen to hear a few somewhat sceptical remarks about the authenticity of these treasures, I can at least honestly vouch for the genuine character of the sumptuous breakfast we found laid out in one of the pleasantest courts of the ruined city. In the same way, when in Russia, after we had for weeks enjoyed unbounded hospitality at the capital, we were invited to Moscow, and there received in the kindest way by the Prince-Governor and the local magnates.

I hope I shall be forgiven for having devoted a few words to what may be termed the festive aspect of these assemblies; but I consider that these agreeable reunions after the day's labour are of importance, and materially help the business of the conference. They tend to bring the delegates together, and many a hitch, which has appeared formidable at the ceremonious meeting in the morning, has been smoothed away by a friendly conversation over the evening cigar. Moreover, I wish to express my hope that the members of the conference of 1879 may be given in London the same cordial reception that they have experienced elsewhere; and I am happy to say that there is little doubt but that the Telegraph Engineers' Society will be well to the front in heartily welcoming our coming guests.

Mr. E. Graves: Will you allow me to propose, what I am sure you will all unite in giving, a hearty vote of thanks to our new President for his address this evening? It is not only on account of the brief and clear *resumé* he has given of the progress of electrical science during the past year that we have to thank him, but particularly for the information concerning the coming Telegraph Conference, and to which I desire to direct your attention. He has shown us how at the previous Conferences the members were received with all respect, and treated with great liberality. Now I believe it will turn out that the reception given to the Conference of 1879 will be *no less* hearty than that awarded

to the preceding ones; but since, unfortunately, our Government traditions do not allow of their preserving, so to speak, so hospitable a front towards our friends as is the case in foreign countries, we must fall back on what I am sure will not desert us, private liberality. And the Society of Telegraph Engineers in supplying the means for honouring the visitors to our capital, will not only be exerting itself, as is fit, in rendering their visit acceptable to them, but will in addition be conferring honour on itself. Later on you will doubtless be desirous of hearing more in detail of the special provision made for the occasion.

Our President will no doubt most ably represent the Indo-European Government Telegraph Department at the Conference, but what is even more important to us, is, that we shall have in him a most efficient representative of the Society of Telegraph Engineers.

Professor W. E. AYRTON: I have the greatest pleasure in seconding the motion, and I would suggest the addition, "and that the address be printed in the Journal of the Society." I have been especially interested with the history of the "word code," described in the address of the President. This word code, I think, is the only method of combating the disadvantages to telegraph companies of packed messages, the existence of which, I believe, sprang to a great extent from the Bolton code. This latter was of course due to the intelligent labours of our Honorary Secretary, whose ill health makes his most unusual absence from the meeting this evening very conspicuous.

In early days the Governments interested in telegraphy seemed to have an idea that packed messages could be put down by legislation; and in fact, in 1871, I was asked to examine packed messages with a view to their suppression. I need not tell you how difficult it was to decide on which were packed messages and which were not, or how unwise would have been any system of suppression. I therefore advised, as I have no doubt many must have advised at the same time, that perfect freedom should be given to everybody to send anything he pleased, and that recourse should be had to a word tariff, so that anyone desiring to send two or three words *would come* direct to the Telegraph Office and do so without going

to a packer. It was of course quite excusable in the earlier days of telegrapy to imagine that a message must be sufficiently long to cover the cost of sending the preamble, but the scientific solution of the difficulty was of course to reduce, if necessary, the length of the official preamble, and not require a sender to pay for a number of words in the body of the message which he had no desire to transmit.

The vote of thanks was then put and carried by acclamation.

Mr. PHILLIPS: I have great pleasure in rising to propose that we accord a vote of thanks to our late President (much applause). It is a very easy task to propose a vote of thanks to Dr. Siemens, because I am sure all the members of this Society have a lively appreciation of his services, and the great assistance he has rendered to this young Society, which was started under the advantages of his name, and we must remember that this is the second time he has been good enough to take the office of President.

Mr. NEWMAN: I have great pleasure in seconding that motion. Dr. Siemens has occupied the presidential chair, very much to the honour of the Society, on two occasions, and has always manifested great interest in its welfare, as well as devoting much valuable time to its interests.

This motion being put to the meeting was carried unanimously.

Dr. SIEMENS (who was warmly greeted) said: I thank you, gentlemen, for the hearty and cordial manner in which you have proposed, seconded, and carried a vote of thanks in my favour. I certainly may claim for myself that I feel a deep interest in the welfare of the Society of Telegraph Engineers. You did me the honour of electing me the first President of the Society at a time when its members did not exceed twenty or thirty in number. At that time I may say I worked with great assiduity with a view of promoting the status and the progress of the Society. You did me the same honour again last year, when its numbers had increased forty-fold; but I cannot claim to have discharged that trust with the same amount of energy as before, for two reasons:—one was that the Society was fairly on its way, and there was not the same necessity for individual exertion on the part of the President, but more particularly so because other engagements prevented me from

attending as much to the business of the Society as I should have wished to have done. The year which has now commenced under the auspices of your new President will be a very important one for this Society. The Telegraph Conference is to be held in London for the first time, and, as Mr. Graves has described to you more eloquently than I could, it is not probable that our Government will do as much in the way of welcoming those illustrious strangers as is the fashion in other countries, but generally speaking this country does not fall short in its ways, which are ways of its own, of showing hospitality and hearty welcome, and this hospitable welcome must proceed in a greater measure from this Society than from any other body. We are the representatives of telegraph engineering, and as such it behoves us to exert ourselves to the utmost to give those strangers welcome, and to prove that we really represent the progress of electrical science in this country. I thank you for your vote of thanks, and I hope under our new President, who is in every way the most qualified man you could have for the ensuing year, this Society will go on and prosper (applause).

The PRESIDENT: I have first to thank you for the kind manner in which you have received my imperfect address, and then to ask you to proceed to the business of the evening, which is the discussion of Major Webber's paper read at the last meeting, and I will ask Professor Ayrton to commence the discussion.

DISCUSSION ON "MULTIPLE AND OTHER TELEGRAPHS AT THE PARIS EXHIBITION."

Professor AYRTON: The paper by Major Webber on "Multiple and other Telegraphs at the Paris Exhibition," before us for discussion this evening, is one of great interest. We have probably all of us been a little mystified with the complexity of cams and levers, but we all know how extremely difficult it is to render clear, with any number of diagrams, the intricate mechanism of a complicated instrument in the absence of the machine itself. Had we time, it might be worth while saying a few words on the cause of this mental inability to image from verbal description.

The most important lesson to be gathered from Major Webber's paper is, that it shows us that on the Continent the electricians are striving in a direction totally different from that in which either the English or American telegraph engineers are aiming. In America, fast speed is attained principally by electrical balance, as in the duplex and quadruplex systems, which, wherever they may have commenced their theoretical existence, certainly entered on their practical life in the United States. In England, duplex and quadruplex are still novelties, and the Wheatstone's Automatic Sender the most prominent instrument at the Post Office. But in France and Italy more use is made of mechanism to facilitate despatch. When visiting the large telegraph office at Naples the other day, it was the prominence given to the Meyer Multiple Sender and the Hughes Type Printer that especially struck my attention; and at Marseilles, again, the rows of Hughes Type Printers were the most prominent feature.

Contrasted with this, I learn that some short time back the last Hughes' instrument in the London Post Office was broken up. Now, it will be most interesting to hear why this great difference exists in the mode of fast speed sending in America, the Continent, and in England. Is it due to the national peculiarities in our characters, or to some difference in the general organisation of the telegraph departments in the different countries?

If I have understood Major Webber's description correctly both Baudot and Schaeffler's apparatus combine the Hughes and the Meyer principle, and in addition overcome the practical difficulties experienced in working these instruments as known in England. The great advantage of the Hughes Type Writer is that the actual message, as printed by the receiving instrument, is sent out to the public, so that there can be no possibility of error on the part of the receiving signaller either in mistaking signals or in copying; on the other hand the disadvantage is that in order to maintain synchronism the printing shaft in the Hughes makes a complete revolution for each letter sent, and as a letter can only be signalled when this shaft is at rest, a delay must occur after each signal before another can be sent, a delay which, in consequence of the time of revolution of the shaft being not more than

seven times that of the printing roller, is equivalent to the time of passing of the chariot over four letters; the simple words, therefore, "be," "fit," &c., &c., although formed of letters in what may be called continuous alphabetical order, cannot either of them be signalled in one revolution of the chariot. The advantage of the Meyer's instrument, as is well known, is that the line is mechanically apportioned for successive intervals of time exclusively to each signaller, in consequence of which the instrument is a quadruplex, and what is more, a quadruplex in which all four messages can, if so desired, be sent in one direction, an arrangement obviously of great advantage in press work, and one not possessed by the American balance quadruplex. The disadvantage I understand is that there is a chance of confusion of the signals in consequence of imperfect separation of the currents, but I cannot say that any mention of this objection was made to me by the officers at Naples, or even of want of synchronism, since in the later forms, of which they had some specimens, of this instrument, there is an automatic correcting arrangement. However, both this objection and that of loss of time in the Hughes are overcome by the "permutation relays" and other devices employed by Baudot and Schaeffler, to which I shall refer further on, but before discussing these instruments I will say a word or two about the instrument of Olsen of Christiania.

There is one point in connection with this instrument which I should feel obliged if Major Webber in his reply would explain, that is, the sending of double letters. If this is done by using double type the idea is I think fallacious. It is very tempting, but at the same time I think fallacious. It will be in the memory of many what vigorous efforts were made by Mr. Walter of the *Times* to introduce his "logotypes" or word types. Not only did he start a printing office for their employment, but even a special logotype foundry when the ordinary typefounders would have nothing more to do with them, and to prove the superiority of logotypes under all circumstances, he commenced publishing and issuing the *Daily Universal Register*, and employed the logotypes for some time even after this newspaper became the *Times*. It seemed at first sight so obvious that if the compositor could pick

out words and not merely single letters, much time must be saved, so it might appear that if on the type-wheel of a printing telegraph we had complete simple words, or at any rate common terminations, such as "ing," "ed," &c., delay in spelling would be avoided. But what was the result, although the word-font system was introduced into Printing House Square by a grand flourish of trumpets, although every opponent was denounced in no mild language as a "false friend and traducer," still even the *Times*, the very newspaper started solely to prove the superiority of logotypes, gradually commenced to be printed with the common letter font. Formerly, not knowing about this, it was a scheme of my own to try word types, or at any rate suffix types in machines such as the Hughes or the mechanical type writer so commonly used now for ordinary correspondence, and it was not until I went to reside in Japan that I realised how unpractical such a scheme must be. For if we *rigidly* stick letters together to stand for a word, or even—and I would attach much importance to this—if we stick lines together to form a letter, we fall into exactly the mistake the Chinese have made for centuries of expressing each idea by a separate sign and not by a combination of signs. The three letters "a-n-d" fastened *inseparably* together to represent the English conjunction constitute as much a Chinese sign as any one they actually possess, and what Chinese signs mean is best realised when you see a set of Chinese type, when you see, as I saw constantly in the printing office of our College, several thousands of boxes all merely constituting one set of font, and, therefore, merely replacing the twenty-five boxes of the English alphabet. Of course, to pick out the right signs in setting-up the compositor had to walk up and down, wasting incomparably more time than would have been necessary to spell out the words.

Now the dot and dash, the Morse alphabet, were probably first used in telegraphy solely because ordinary letters could not be signalled; but just as an enormous advance was made in literature when signs for each idea were replaced by a comparatively few letters making words, so I am inclined to think a second great advance was made when ideas, however complicated, were expressed by combinations of two signals only. And this, I may venture to

suggest, may be one of the principal reasons why in the regular telegraph offices of countries like England and America, where speed is desired, no type writing telegraph has found favour, however useful such instruments may be on the semi-private lines of the Stock Exchange or the Fire Brigade.

Similarly, it appears to me wise that the English Post Office has not adopted any form of telephone for the ordinary mass of messages, since what we want is speed, not speech; therefore, for the main traffic it would be impossible to economically utilise the large staff of already trained Morse-signallers in sending and receiving with any form of telephone, even if (as I think is quite practicable) all difficulties regarding induction were cleared away.

Now, how has the delay in what I may call "character signalling"—I mean sending letters, and not dots and dashes—been met in the Baudot five message instrument? Certainly most ingeniously; by a device through which the letter sent may be printed at the distant station while the line is being employed in transmitting a totally different letter from another sending instrument: in fact, a signal is instantaneously sent and stored up, and the corresponding letter not printed off until the proper rubber comes over the right portion of the proper combiner. The time, therefore, during which in the ordinary Hughes instrument the line is idle, while the chariot is revolving, is employed by M. Baudot for the other instruments to signal along the line.

But although this instrument as well as that of M. Schaeffler is extremely ingenious, it will, I think, be regarded as too complicated for the English lines, and a similar remark must, I am afraid, be made regarding the instruments of M. D'Arlincourt, shown us at the last meeting of the Society. These copying telegraph instruments have an extremely pretty device for producing synchronism consisting of the sending instrument being stopped at each revolution of the cylinder on which the sent message is wound, and re-started by a current sent at every revolution of the receiving cylinder when this arrives at a fixed position corresponding with that in which the sending cylinder has been stopped. But it is this very correcting arrangement which is the weak point of the instrument, for in order to prevent loss of time it is necessary that

the sending cylinder should take up its motion each time suddenly so as to restart with the velocity it had on stopping. This necessitates two sets of clockwork in each instrument, one for maintaining synchronism, and the other for driving the cylinders; the one always going, the other sometimes disconnected and at rest, while at other times it is suddenly connected, and restarts at full speed. Now the inertia of bodies makes this somewhat difficult, and so recourse is had to a revolving arm of aluminium, a weak metal, unfortunately, and so this arm is frequently bent. The consequence is, that although I regard these fac-simile instruments as models of ingenuity, and although it is possible in a few minutes to telegraph with them a survey which would take pages of writing if described in words, still the experience that I have had of them for some years in Japan would not induce me to recommend them for ordinary telegraph lines. But it is very important that we should carefully distinguish between our dislike to an instrument because it is complicated and likely to get out of order, and our dislike to it because we as a people are extremely conservative, and take pleasure in following beaten tracks, and therefore shun novelties. Now it does not appear that this objection of complexity is shared by Elisha Gray's harmonic telegraph, and yet I am not aware that this harmonic principle, advocated even in 1870 by Mr. Cromwell Varley, has been utilised in practice. Consequently, although fully sensible of the advantages accruing from any large scheme, like the telegraphs of a country being carried out by any single large company like a Government, still I cannot but think the feeling that if any one puts up telegraphs he may be trespassing on Government preserves, may to a certain extent prevent enterprise. I do not wish it, however, to be concluded that I am utterly adverse to a Government monopoly. While one is reading Herbert Spencer's works one is of course convinced that it is bad, but on the other hand it is a grand thing to be able to send for a shilling a telegram from a small village up to London, which one could never do in the companies' days. It is a very bad state of things, undoubtedly, if any Government monopoly makes the service shun an inventor, but I am not sure that the fierce competition between railway companies, which causes money to be wasted

by two companies bidding against one another, to buy up some comparatively unimportant proposed small railway, is not worse.

Our President has this evening clearly explained to us why it is impossible at present to send telegrams as cheaply as letters, viz.: because "telegrams must be manipulated one by one." But may this not only be a result of the present absence of machinery in telegraphing; and does not Major Webber's paper lead us to look hopefully forward to the day when the real labour of telegraphing shall be performed by the public, as in the case of letters, and the duty of the department will merely consist in transmitting the telegrams in a comparatively wholesale manner?

Sir CHARLES BRIGHT: I have very few remarks to make upon the instruments of which Major Webber has spoken, viz., the multiple system, with respect to which I am in accord with the last speaker that, in my opinion, they are not suitable for the general purposes of a telegraph office, however far they may work experimentally. The direction of the process employed by Baudot, Schaeffler, &c., is to split up a second into five parts, to be distributed amongst five different instruments, and I do not believe that in the general work of a telegraph office such a system would be effective. For my own part, I would rather urge upon inventors to improve the speed of working of the particular apparatus which they use—or anything rather than split up five Hughes instruments to take up different portions of a second by what I consider a very complicated division. I think Professor Ayrton is wrong in saying one revolution is required for every letter in the Hughes instrument. One revolution would be required if you could not pick up successive letters; but, as a fact, I believe as many as two letters are got out of Hughes instrument in the course of a revolution. I have seen Brett's combination instrument which was brought out in America; I also saw the Hughes instrument when it was introduced in 1858; and I believe I am fairly well acquainted with everything concerning type-printing instruments, including that of our honoured member, Dr. Siemens; and to my mind they have all this fault—that they waste a great deal of time in passing over letters that are very rarely used. Some letters are used much more than others. Those chiefly used in our

language are c, e, n, o, t, and s, and a blank, *i.e.*, the open space which occurs 900 times, and e 600 in 5,000 letters. There is great waste of time in all type instruments in going over so often those letters which are rarely used ; so that it appears to me a mistake to attempt to divide a number of type-instruments into such small divisions of time, as is the case with the Baudot and the Meyer's instruments. Professor Ayrton spoke of the Walter logotypes. Those would be impracticable for telegraph operations, because we could not hope to get a thousand different divisions of a type for words that are principally used. Type-writers, as Professor Ayrton says, have never found favour because they are too slow, but I think we may hope to see an automatic type instrument which will work very rapidly. Endeavours have been made, and I think human ingenuity may carry out, a type-printing instrument which will work very much quicker than any kind of multiple apparatus. And although experiments have shown that the Baudot system may be used satisfactorily for experimental purposes, yet I doubt whether it could be practically adapted for daily use in telegraph offices.

Professor AYRTON, in reply to Sir Charles Bright, said he was afraid he had been somewhat misunderstood if it was thought that he stated in his previous remarks that in each revolution of the type-wheel of the Hughes instrument one letter only could be sent. What he desired to convey was that for each letter sent it was necessary to have one complete revolution of the *printing shaft*, the shaft that carried the cams, and as the periodic time of revolution of the printing shaft in the ordinary Hughes instrument was one-seventh of that of the chariot or type-wheel, it followed that after each letter transmitted four had to be slipped. The word "cot," for example, could be signalled in one revolution of the chariot, since there were more than four letters between "c" and "o," and more than four between "o" and "t," but the simpler word "be" required two revolutions.

Mr. AYRTON further mentioned that he had instanced the logotypes not with a view of introducing them into telegraphy, but because he considered they explained the superiority of a dot and dash system over an alphabetical telegraph, and because he was

anxious to hear from Major Webber whether the double letter sending in Olsen's machine was accomplished by any such device.

Mr. W. H. Preece: I will say two words in order to disabuse the minds of the meeting of the idea that the Post Office have been such Goths and Vandals as to destroy the last Hughes instrument. The fact is we have sixty-three Hughes instruments, and should be glad to dispose of them to Professor Ayrton or to anybody else. More than that, the Submarine Company employ the Hughes instrument largely in conducting telegraph communication with the Continent; and anyone who visits the chief station of the Submarine Company will find an army of Hughes instruments far exceeding in number those at any station this side of Marseilles or any station out of Paris.

Major Webber: The hour is so late that I shall be as brief in my reply as possible. In the first place, I will ask members to read over the paper which I had the honour to bring to their notice at the last meeting—and I believe it will be distributed in the course of a few days—because in it I have made additions, and also there will appear certain portions of the description which I attempted to give of the apparatus of Baudot and Schaeffler, which time did not permit me to read at the meeting. I quite agree with Professor Ayrton that there is difficulty with the somewhat imperfect diagrams such as mine were in enabling a meeting to understand such complicated arrangements as are comprised in apparatus of this kind, but I hope that my descriptions when read, with the help of the published diagrams, will be found sufficiently clear to the student. I may here remark that they are quite original, compiled from the apparatus exhibited, and that their descriptions are the first which have been written of them in the English language.

As regards the Olsen apparatus, of which I had only time to read a brief description, but about which more will appear in print, I will only add that M. Olsen has improved on the Hughes apparatus, as he can print five characters in one revolution of the sledge in place of three. I would also remind Sir Charles Bright the Olsen is an automatic printer, although his speed must

always be limited on account of the great length of the slip of paper which he uses for signalling automatically.

At Paris Sir Charles Bright noticed the composing chain of Girarbon, an illustration of which appears in the paper, and was struck with the promise it gave of one day increasing the speed of type printers, as mentioned by him to-night. I think the meeting generally would be inclined to agree with Sir Charles Bright that it would be a great mistake to divide up the Hughes printer. That is not what I wished you to understand was the Baudot system. He has practically five type printers on one line of communication, worked by pulsatory currents, which are emitted at the receiving end of the line. Sir Charles Bright thinks these currents are likely to become what the Americans call solid currents, but I do not think the French Government would spend so much money over the invention if there was not a prospect of its succeeding. A very large number of these currents sent following one another on one line can be distributed amongst five or six instruments. I would say a word about type printed telegrams. Nothing has been said to-night to suggest a reason why the French public seem to demand to have their telegrams printed, or why there is not a similar demand made on the part of the public in this country. I think it is probable that instead of being a matter of demand from the public, the French Administration considers that the telegrams are more accurately transmitted when they are printed; while, on the other hand, the English Administration has proved to the public that great accuracy can be obtained without using type printers, and I think that is a simple solution of the question. In fact, it is more a question of what the Administration will supply than of what the public demands. There is another difference of importance. The telegraph in France does not extend to small places as is the case in this country. The French *employés*, while as a rule not so highly paid as in England, have more mechanical intelligence than would be found amongst those doing the same work in this country; at the same time, I am the last man to allow that the English telegraph is in his own way not the superior of his French *confrère*. But if the demand for type printing ever arose in this country, I believe it is quite possible

to obtain instruments which could be used in place of the Wheatstone alphabetical instrument, and which could be worked as easily—which would not be more costly, and which would produce type printed messages without having to employ persons of greater mechanical or technical knowledge than those who now work the Wheatstone instrument in small villages under the Post Office system. From a conversation I had with M. Baudot, I gathered that he has studied the subject for several years, and he hoped to bring before the telegraph world an instrument of that sort, and perhaps when that is done the British Administration will supply the public with type printed messages as well as the French. But the real object of multiple telegraphs is to solve the question of how to get the greatest amount of work in a given time through one wire, and it is in such countries as England, France, and America, and on the main lines alone, that this question becomes of vital importance from an economical point of view.

It is in the nature of telegraphy that the more successfully this is met by speedy transmission at the busy hours, the more the pressure at those times increases. Every new invention, which will successfully meet this pressure will therefore be gladly hailed by telegraph engineers as the immediate outcome of their daily wants.

The PRESIDENT: On the last occasion we thanked Major Webber for his admirable paper, and I am sure we must now thank him for the remarks he has just made, and we must also feel grateful to Professor Ayrton, Sir Charles Bright, and to those who have taken part in this interesting subject. I had an opportunity of glancing for a few minutes over Major Webber's paper as printed in the Journal, and I should like you to notice how admirably, thanks to Professor Ayrton, the drawings are executed. This removes in a great degree the difficulty of describing verbally such machinery as this in the absence of the machinery itself.

ABSTRACT.

ON MR. L. VIANISI'S DUPLEX SYSTEM.

By Dr. A. Tobler,
Lecturer at the Polytechnic School of Zurich.

(Translated from Dingler's Polytechnic Journal, Vol. 227, 1878, by the author.)

One of the most remarkable systems of duplex telegraphy for simplicity and efficiency, is that described by Mr. L. Vianisi, Inspector of Telegraphs at Messina, in the *Journal Télégraphique*, 1874 and 1876. In principle it is based, like the system of Mr. Geritt Smith, Assistant Electrician to the Western Union Telegraph Company, on what is called Poggendorff's bridge.

Mr. Vianisi has brought forth no less than nine different combinations of his system, of which the arrangement No. 6 (*vide Journal Télégraphique*, 1876, p. 319) is in successful operation in Italy and Switzerland. It differs from the others in effecting the balance in the receiver of the sending station, by means of a current coming from the receiving station; therefore each station needs but one battery (a similar arrangement has been designed by Mr. G. K. Winter, Telegraph Engineer to the Madras Railway). When neither key is depressed, both batteries are closed, but since similar poles are connected to the line, the currents being equal and opposite, do not produce any effect. When station I is sending, its own battery is short-circuited through I's receiver; when both I and II send, each battery acts upon its own receiver, the currents passing through the line neutralizing one another.

Before describing the detailed arrangement, let us consider for a moment the mathematical basis of the question, which has been studied by Mr. Vianisi.

Let the opposite poles of two batteries P and P′ (figure 1)

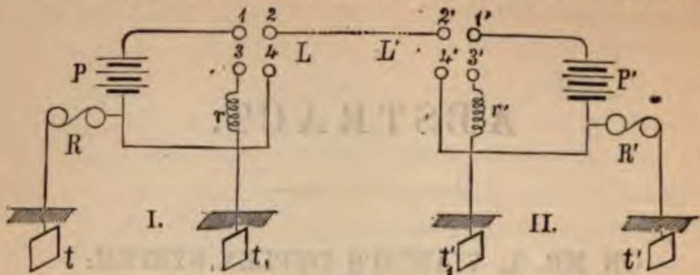


FIG. 1.

whose electromotive forces are E_1 and E_2, be connected by wires, and further, a receiver or galvanometer be connected at each station. If the strengths of current in the three branches be called respectively J_1, J_2, J_3 (figure 2), and the resistances W_1, W_2, W_3, the application of Kirchhoff's laws gives the well-known equations.

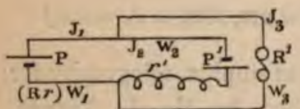


FIG 2.

$$J_1 - J_2 - J_3 = 0. \quad 1.$$
$$E_1 = J_1 W_1 + J_3 W_3. \quad 2.$$
$$E_2 = J_2 W_2 - J_3 W_3. \quad 3.$$

From these equations we deduce the values of J_1, J_2, J_3, *i.e.*,

$$J_3 = J_1 - J_2 = \frac{E_1 W_2 - E_2 W_1}{W_1 W_2 + W_2 W_3 + W_1 W_3} \quad . \quad . \quad . \quad . \quad 4.$$

$$J_1 = \frac{E_1 - J_3 W_3}{W_1} = \frac{E_1 (W_2 + W_3) + E_2 W_3}{W_1 W_2 + W_2 W_3 + W_1 W_3} = \frac{E_2 + J_3 (W_2 + W_3)}{W_2}. \quad 5.$$

$$J_2 = \frac{E_2 + J_3 W_3}{W_2} = \frac{E_2 (W_1 + W_3) + E_1 W_3}{W_1 W_2 + W_2 W_3 + W_1 W_3} = \frac{E_1 - J_3 (W_1 + W_3)}{W_1}. \quad 6.$$

If $E_1 W_2 - E_2 W_1 = 0$, then $J_3 = 0$ . . . . 7.

These equations will be made use of in the case when one station is sending without receiving at the same time.

The key of a peculiar construction, as designed by Mr. Vianisi, has, when depressed, to break two circuits, and to establish two different ones without interrupting the line. The first key made has been described in the *Journal Télégraphique*, 1874. Figs.

3 and 4 represent the improved instrument, as constructed by Messrs. Hasler & Escher, of Berne.

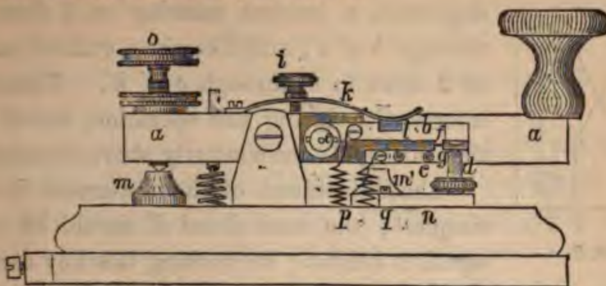


FIG. 3.

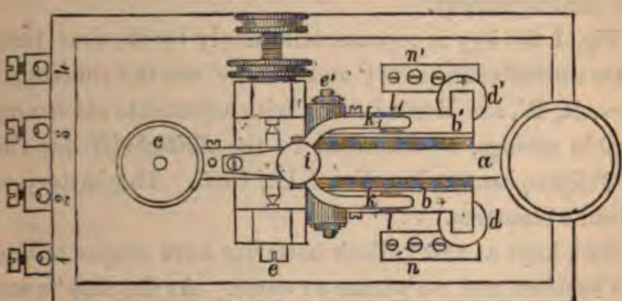


FIG. 4.

The main key-lever is provided with two auxiliary levers, b and b', which are insulated by ebonite plates from the main lever, and are movable round the axes, e and e'. When the key is at rest, the auxiliary levers are pressed on the insulated contact pieces, c and c', by means of the forked spring, k, k'. Connection between the fork and the levers is avoided by the use of ebonite pieces, l and l_1. The wooden ground-plate carries two adjustable contact studs, d and d'. If we now depress the key, the auxiliary levers come in contact with the studs d and d', and lose finally their connection with c and c'. In releasing the knob, b and b' are first connected with c and c', and at last the connection of b and b' with d and d' is broken.

The connection between the binding screws 1-4, and the auxiliary levers and contact pieces, may be seen in Fig. 5.

When the key is at rest, the terminals 1 and 2 through b and c, 3 and 4 through b' and c', are connected. When the key is depressed, a current entering at 1 flows to 3, through $b\ d\ c'$, and likewise a current entering at 2 flows to 4, through $c\ d'\ b'$. There is no interruption during this operation, but of course the battery is momentarily short-circuited.

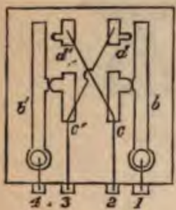


FIG. 5.

The adjustment of the apparatus is of the simplest; the studs d and d' should be exactly equal in height. In working the key care must be taken not to spoil the thin silk-covered spiral wires which establish the connections between the levers and contact pieces and the binding screws.

In Fig. 1 the key is represented merely by the four terminals, which are connected in pairs; and r and r' are the rheostats. The receivers, R, R′, are Morse inkers, with adjustable electro-magnets and double springs, as described in the *Zeitschrift des Deutsch-oesterr Telegraphenvereins*, Vol. XII., 1865. The battery consists of Callaud's elements.

1. Both keys at rest. Both batteries have copper to line, zinc through receiver and resistance to earth. As the line is supposed to be perfectly insulated, no current will pass.

2. Key at station II depressed, at I at rest. The + current of the battery P (at station I) goes through the line L to II, passes through the parallel branches R′ and r', and returns through earth to I by way of R and r. But the receiver at II (the sending station) is at the same time under the influence of its own battery, P′, whose + current enters the receiver R′ from below, so a balance may be effected in R′. This arrangement is shown in Fig. 2, where W_1 represents the joint resistance of the line I, the battery P, the branches R and r, and the earth; W_2 represents P′, and the rheostat r'; and, finally, W_3 represents R′, the resistance of the sending station's receiver. If it were possible to make $W_1 = W_2$ (E_1 is supposed *a priori* $= E_2$), the balance in R′ would be strictly established (see equation 7). This is usually not the case, the receiver of the sending station being under the influence of a

feeble current, which, however, is incapable of producing any effect.

3. Both keys depressed. The + current passes in both stations through rheostat and receiver to the zinc pole. The zinc pole of both batteries is moreover connected to line; the currents neutralise therefore each another in the line.

Let us now choose a practical example in order to see whether any difference exists between the single and duplex signals.

Let the resistance of the line be 1,200 S. U., and of the receiver 400 S. U. Let the resistance in each rheostat be 1,600 S. U. (Line plus receiver.) The battery may consist of 18 Callaud elements* (at each station) whose electromotive force is say 18, and whose joint resistance is 108 S. U. (electromotive force of one element = 1; resistance = 6 S. U.)

In case No. 2 we have according to equations 4 — 6,

$$W_1 = 1200 + 108 + \frac{400 + 1600}{400 \times 1600} = 1628$$

$$W_2 = 1600 + 108 = 1708$$

$$W_3 = 400$$

$$J_3 = \frac{18 \times 1708 - 18 \times 1628}{1628 \times 1708 + 1708 \times 400 + 1628 \times 400} = 0{\cdot}00035$$

$$J_1 = \frac{18 + 0{\cdot}00035\,(1708 + 400)}{1708} = 0{\cdot}0109$$

$$J_2 = \frac{18 - 0{\cdot}00035\,(1628 + 400)}{1628} = 0{\cdot}0106$$

Since the rheostat r forms a shunt to the receiver R, only $\frac{4}{5}$ths of the current J_1 will flow through R, for

$$\frac{R}{r} = \frac{400}{1600} = \frac{1}{4}$$

and

$$\tfrac{1}{4}\,J_1 = \tfrac{4}{5} \times 0{\cdot}0109 = 0{\cdot}0087.$$

Let the magnetic moment of the receiving magnet be equal to the strength of current multiplied by the square root of the mag-

* These data very nearly represent the Zurich-Schaffhausen duplex circuit.

net's resistance (*vide* Weidenbach, *Compendium der Telegraphie*, Wiesbaden, 1877), then we have

$$M_1 = \tfrac{4}{5}\, J_1 \sqrt{W_3},$$
$$= 0{\cdot}0087 \times 20,$$
$$= 0{\cdot}1740.$$

The receiver of the sending station receives a magnetic moment, 20 J_3 or 0·00700 which is entirely incapable of producing any effect.

In case No. 3 we have for each station

$$J = \frac{18}{1600 + 400 + 108} = 0{\cdot}0085;$$
$$M_3 = 0{\cdot}0085 \sqrt{400} = 0{\cdot}1700.$$

The difference of the magnetic moments in cases No. 2 and No. 3 is equal to D or 0·004, which is a very good result. Therefore, the simple and duplex signals are nearly of equal strength.

In Mr. Vianisi's primary arrangement (*vide Journal Télégr*, 1876, p. 234) the terminals 3 and 4 were insulated when the key was at rest, therefore the current acted with full force on the receiver at I, since the shunt *r* did not exist. The consequence was that W_1 could be made equal to W_2, and therefore the current J_3 and magnetic moment in $R^1 = O$.

When we apply the values used before to this latter arrangement we find for case No. 2 the magnetic moment of R^1,

$$M_1 = 0{\cdot}2100.$$

In case No. 3 we find, as above stated,

$$M_3 = 0{\cdot}1700$$
$$\therefore D_1 = 0{\cdot}04,$$

i.e., considerably greater than in the former example. The experiments made by Mr. Vianisi on the Messina-Palermo and Messina-Catania circuits show, however, that receivers which are not too sensitive will work well enough with this arrangement.

The difference between the simple and duplex signals in our first example may be still more reduced by inserting a resistance-

coil of 40 S. U. at each station between the terminal 1 and the line, we find then for case No. 2—

$$\begin{aligned} J_3 &= 0 \\ J_1 = J_2 &= 0{\cdot}0105 \\ M_1 &= \tfrac{4}{5} J_1 \sqrt{W_3} \\ &= 0{\cdot}1680\,; \end{aligned}$$

for case No. 3, as above stated,

$$\begin{aligned} M_3 &= 0{\cdot}1700 \\ D_2 &= 0{\cdot}002. \end{aligned}$$

It may be mentioned that it is the usual practice to insert a small galvanometer (detector) between the receiver and the earth; the resistance of it (in our country only 2·5 S. U.) has to be taken into consideration in the numerical examples.

The receiver of the sending station shows a certain inertia. In order to experiment upon this I made an arrangement as in Fig. 1. The receiver was—1st. A relay magnet constructed by Dr. Hipp, of Neuchâtel. 2nd. A Siemens polarised relay. 3rd. A Hughes instrument. In order to make R work, the following alterations of W_1 were necessary (W_1 and W_2 were originally rheostats of 500 S. U. each): 1st. W_1 = 320 S. U. 2nd. W_1 = 360 S. U. 3rd. W_1 = 460 S. U. But in the last case the Hughes magnet was adjusted to a sensitiveness which would hardly suit in practice.

Mr. Vianisi's duplex method may also be applied to Hughes' type-printer. Mr. Vianisi proposes for this purpose his automatic repeater, a relay whose contact lever acts in the same manner as the key described above. It is then inserted in the (local) circuit of the sending Hughes. *But I venture to express the opinion that the modern improvements of the Hughes*, i.e., *the mechanical locking of the printing axis* (*vide Journal Télégr*, 1876, and *The Telegraphic Journal*, 1877, p. 160) *render it possible to dispense with an auxiliary apparatus.* It is only necessary to fit the rocking lever of the sending Hughes with two contact levers similar to those accompanying the hand-key, but of a lighter construction. Two instruments will be needed for each station.

In the above quoted remarkable work of Mr. Weidenbach,

there is a numerical example given relating to the bridge method, as modified and applied to the Hughes by Messrs. Grimmert & Canter. In order to draw a parallel between the two, let us adopt the same numerical values. Resistance of line = 80 r (all the resistances are expressed in "Rheostat-Units," each of them equal to 50 S. U.), resistance of one Hughes = 20 r, strength of current necessary to work one Hughes = 0·73; electromotive force of one element = 1, resistance = 0·12 r. It is said there that under normal circumstances about 96 elements are required for working a Hughes (single circuit) on a line of 80 r. Let us now assume 100 elements, the application of the above-noted formulæ gives the following results:—

$$J_3 = \frac{100 \times 112 - 100 \times 108{\cdot}6}{112 \times 108{\cdot}6 + 112 \times 20 + 108{\cdot}6 \times 20} = 0{\cdot}0205$$

$$J_1 = \frac{100 + 0{\cdot}0205\ (112 + 20)}{112} = 0{\cdot}917$$

$$J_2 = \frac{100 - 0{\cdot}0205\ (108{\cdot}6 + 20)}{108{\cdot}6} = 0{\cdot}8965$$

Since, just as in the first example, the rheostat forms a shunt to the Hughes magnet, $\frac{5}{6}$ J will flow through the coils (or $\frac{5}{6} \times 0{\cdot}917$, that is 0·764). But the moment the armature of the receiving Hughes strikes the locking lever, the coils are short-circuited, and the values just obtained are altered, *i.e.*,

$$J_3 = \frac{100 \cdot 112 - 100 \cdot 92}{112 \times 92 + 112 \times 20 + 92 \times 20} = 0{\cdot}139$$

$$J_1 = \frac{100 + 0{\cdot}139\ (112 + 20)}{112} = 1{\cdot}056$$

$$J_2 = \frac{100 - 0{\cdot}139\ (92 + 20)}{92} = 0{\cdot}917$$

The current J_3, which flows through the receiving Hughes of the sending station, evidently strengthens the induced magnetism of the cores, since it flows in a direction opposite to that which produces the signals at II. If, now, station I begins to send, a sudden reversal takes place in the magnet at station II. An experimental investigation of these facts seemed interesting to me,

for it might be apprehended that the magnet would show a certain inertia. I connected a Hughes with two batteries and two rheostats, as shown in Fig. 1; between P and P′ a key was inserted whose depression suddenly broke the circuit of the battery P. Having first made $W_1 = W_2$, I gradually diminished W_1, which had the effect of strengthening the cores in R, and suddenly depressed the key, then the magnet was solely acted on by P′. When the magnet was properly adjusted, the locking of the printing axis was effected without the slightest delay or trouble.

In case No. 3 the current acting on each Hughes is—

$$J = \frac{100}{100+20+12} = 0{\cdot}757$$

and the difference of currents in cases No. 2 and No. 3 = 0·007.

J_3 could be made = 0, by a proceeding which has been mentioned before, *i.e.*, by the insertion of a resistance coil between 1 and the line.

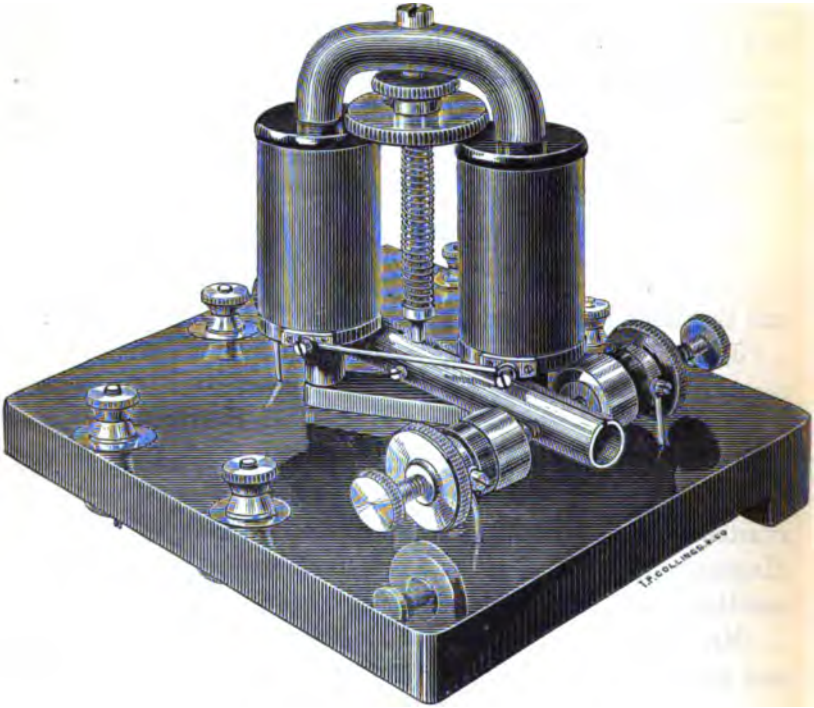
In the example given by Mr. Weidenbach there are no less than 333 elements required to obtain a current, J = 0·73. But it must be remembered that the bridge method may be applied to Hughes instruments of old and modern construction, and that no auxiliary mechanism need be added.

Mr. Vianisi has also indicated a method of duplex translation, and sundry dispositions to avoid the bad effect of the discharge, as occurs on lines of greater length. They are to be found in the *Journ. Télégr.*, 1876, but it is not my intention to give a criticism of them, as it would unduly lengthen my paper.

ORIGINAL COMMUNICATIONS.

A NEW FORM OF SOUNDER.

By G. Dubern.



The accompanying figure represents a Sounder for direct working or translation, and of which over one hundred have been constructed for the telegraph lines in India.

It differs from an ordinary Sounder in the arrangement of the armature, which is supported by a vertical spiral spring. This spring serves to restore the position of the armature when the current ceases, as well as to relieve the lower bearing of nearly all friction. The prolongation of the armature acts as the beam, and plays between the two stops shown in the figure.

To ensure very good insulation for damp climates, the coils are rendered solid by repeated immersions in a compound of resin and bees-wax (for hot climates, ten parts of resin and one of wax answer well).

The range of the instrument is shown by it working without fresh adjustment, either with one Daniell's cell through 6,000 ohms or with 20 through 0. When very delicately adjusted, one Daniell's cell through 31,000 ohms is just able to work the instrument if the stops be extremely close together.

G. DUBERN.

Calcutta, October 14th, 1878.

NOTE TO THE ARTICLE ON ELECTRO-MAGNETS, &c.

BY OLIVER HEAVISIDE.

In Nos. XXII., XXIII., of the Journal, Vol. VII.

In the second part of paragragh 7 of my article on Electro-Magnets in the last number of this Journal, I stated :—" Since m is proportional to the pitch, for sufficiently high pitches Γ is inversely proportional to the pitch." This is an error; or rather, it is true, but does not correctly apply to the case then considered, viz: the Bell Telephone. It is true for a constant electromotive force, thus: the amplitude of very rapid reversals from a battery through an electro-magnet is inversely proportional to the pitch, and so it will apply to a battery telephone. But in an electro-magnetic telephone, although the electromotive *impulse* produced by a semi-vibration of the iron disc of the sending telephone is *constant* for all pitches, provided the amplitude of the vibration is the same, yet when the semi-vibration is executed in half the time, the mean value of the electromotive force is doubled. The result is that a tone of double pitch is reproduced of rather geater intensity, instead of rather more than half the intensity. Thus, Γ being the current, R the resistance, L the electro-magnetic capacity, and $m=\frac{2\pi}{T}$, when T is the time of a complete vibration,

$$\Gamma=\frac{m\,E}{\sqrt{R^2+L^2m^2}}$$

in the case of a bell telephone, where E is proportional to the amplitude of vibration of the sending disc. Keeping E constant, Γ increases slightly with m. Also, when R is increased, Γ is reduced more for low pitches than for high.

OLIVER HEAVISIDE.

STATEMENT OF RECEIPTS AND EXPENDITURE FOR THE YEAR 1878.

RECEIPTS.

	£	s.	d.	£	s.	d.
By Balance, December 31st, 1877	102	17	10			
" Uncashed Cheque, 1877	10	0	0			
" Subscriptions paid up for previous years	196	0	0			
				308	17	10
" Subscriptions for 1878				728	15	6
" " paid in advance				28	8	0
" Compositions of Life Subscriptions				71	0	0
" Publishing Fund				7	15	6
" Sale of Journals				204	14	1
" Entrance Fees				19	18	0
" Profit on Remittance from America				0	14	5
" Cheque returned by Bankers				6	5	0
" Subscriptions received on account of Conference Reception Fund				57	8	0
" Subscriptions for Ronalds' Catalogue				6	18	0
Total				£1,435	14	4

EXPENDITURE.

	£	s.	d.	£	s.	d.
To Unliquidated Liabilities, 1876	40	9	6			
" " " 1877	339	1	3			
				379	10	9
" Salaries, Clerical assistance and Draughtsman	129	7	7			
" Shorthand Reporter	18	18	0			
" Attendance and Refreshments at Meetings	16	4	8			
" Printing and Stationery	239	11	11			
" Lithographing	36	7	0			
" Ronalds' Library, preparing Catalogue and Insurance	72	16	5			
" Rent, Taxes, and Fuel	129	6	7			
" Petty Expenses	139	6	6			
" Returned Subscriptions	4	9	6			
" Cheque returned by Bankers, and Bankers' Charges	7	18	0			
				794	1	2
" Conference Reception Fund Banking Account ...				57	8	0
" Balance *Cr.* (of which £6 18s. is paid in advance for Ronalds' Catalogue, leaving nett available balance, for current expenditure, £197 16s. 5d.)				204	14	5
				£1,435	14	4

We have compared the above Account with the Vouchers and Cash Books, and find it to be correct, leaving in the hands of the Bankers Two Hundred and Four Pounds, Fourteen Shillings, and Five Pence.

FRED. CHAS. DANVERS.
J. WAGSTAFF BLUNDELL.

CONFERENCE RECEPTION FUND ACCOUNT.

	£	s.	d.
By Subscriptions	57	8	0
Total	£57	8	0

	£	s.	d.
To Subscriptions returned, as per Bank Book ...	5	15	6
" Balance *Cr.*	51	12	6
	£57	8	0

ESTIMATE OF ASSETS AND LIABILITIES ON THE 31ST DECEMBER, 1878.

ASSETS.

	£	s.	d.
To Balance at Bankers (including £6 18s. 0d. paid in for Ronalds' Catalogue)	204	14	5
„ Unpaid Subscriptions, estimated	412	9	6
„ Estimated value of furniture	200	0	0
„ Objects presented to the Society (valued at £225, but this sum is not commercially realisable)	—		
„ Journals in hand, estimated relisable value...	563	18	0
Total ...	£1,381	1	11

LIABILITIES.

	£	s.	d.
By Salaries and Clerical Assistance	25	6	6
„ Shorthand Reporter	4	14	6
„ Attendance and Refreshments at Meetings ...	5	6	0
„ Printing and Stationery (including cost incurred on Ronalds' Catalogue to date) ...	*833	7	7
„ Ronalds' Library, preparing Catalogue and Insurance	6	6	0
„ Rent, Taxes, and Fuel	43	19	4
„ Petty Expenses	2	15	1
„ Subscriptions paid in advance	23	8	0
„ Balance *Cr.*	435	18	11
	£1,381	1	11

E. GRAVES, *Honorary Treasurer.*

* £675 has been advanced by certain Members of Council, as a loan, to enable the Society to discharge its Printer's bill to the end of 1878.

INDEX.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. VIII. 1879. No. 26.

The Seventy-Third Ordinary General Meeting of the Society was held on Wednesday evening, February 12th, 1879, Lieut.-Colonel BATEMAN-CHAMPAIN, President, in the Chair.

After the preliminary business had been disposed of, the President called on Mr. Willoughby Smith to read his Paper on

THE WORKING OF LONG SUBMARINE CABLES,

which he proceeded to do as follows:—

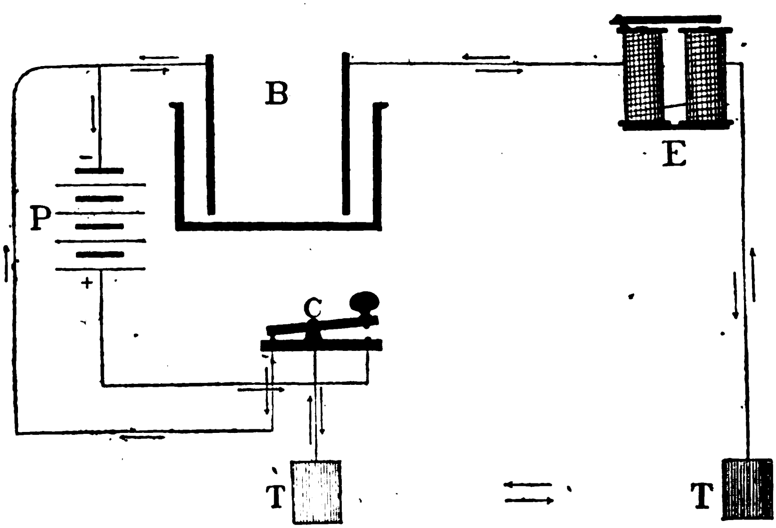
It has occurred to me that it would be interesting to the members of this Society, as well as useful to them for purposes of reference, to put together a brief history of the working of long submarine cables since the present system came into operation, now some twelve years ago. I have been the more induced to do this, as there seems to be very considerable misapprehension existing as to how and when and why the present system was introduced, and many strange improvements (so-called) have been brought under my notice from time to time, which the inventors would surely never have worried themselves so much about if they had been acquainted with the real state of the case. Having had to do with the subject personally and practically ever since the laying of the first successful Atlantic cable, I am perhaps in a position to give an accurate and authentic statement of what has occurred; and my paper to-night will be, as I have indicated, little more than a connected statement of facts, commencing with what I believe was about the earliest and crudest development of

the idea which has now been so effectually carried out, though in a totally different form.

I also propose to take this opportunity of giving you the results of a few experiments I have made in one or two of the more modern branches of the subject, bearing as they do upon the main question I have in hand.

When electro-magnets were introduced into the instruments used for working long aêrial, subterranean, or submarine circuits, it was desirable to prevent as far as possible the prejudicial effects of the residual magnetism in the same. Among the many methods suggested to accomplish this was one known as "The Counter-current System" of M. Jacobi. To overcome what he termed "the injurious effects of condensed magnetism" he introduced a polarisation battery into the circuit, so that, "if the electro-magnet has preserved its attractive action, the latter finds itself destroyed by the secondary current; and as the polarisation current varies in intensity with the current of the working battery, the system being once properly adjusted, the injurious effects of the condensed and residual magnetism are always counteracted by a force which increases or diminishes with them." The polarising battery

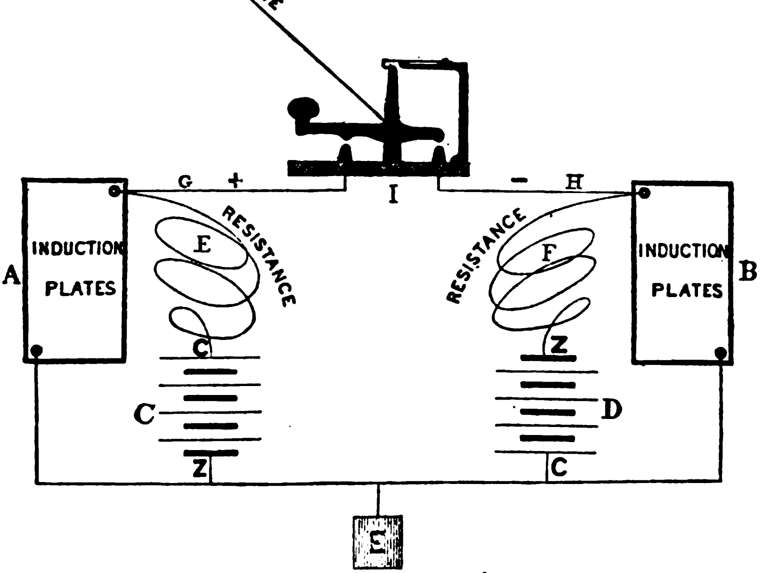
I



consisted of plates of platinum immersed in sulphuric acid, and was successfully used on the line between St. Petersburgh and Moscow. The connections were as shown in the diagram No. 1: B is the polarisation battery, E the receiving instrument, C an ordinary Morse key, and P the working battery.

In 1860 Mr. C. F. Varley patented "Improvements in Electrical Telegraphs" in reference to long submarine or subterranean lines, in which he suggested the use of polarising batteries, condensers, or induction plates as a substitute for the same. In this patent Mr. Varley says, "For a really instantaneous discharge into the cable the tension must be indefinitely great to give a signal of sufficient force," and as such tensions endanger the cable, he preferred, instead of an induction coil, to charge condensers of known capacity, and discharge them in connection with the battery into the cable. Diagram No. 2 will make the arrangement clear without any further explanation.

2

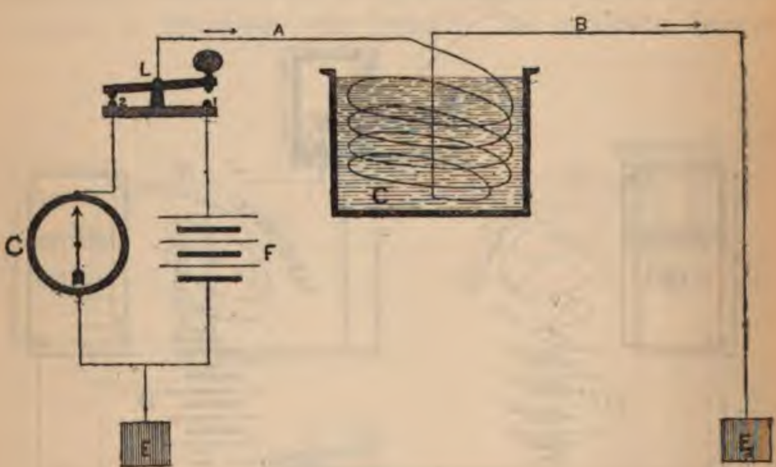


At the date of this patent Mr. Varley was electrician to the Atlantic Telegraph Co., and a few weeks previously had informed

that Company that to insure eight or nine words per minute through an Atlantic cable the conductor must weigh 1,150 lbs., and the gutta percha 1,020 lbs. per knot, and that a magneto-machine worked by a donkey engine would be found to be one of the best sources of electricity for working such a cable.

On the 14th February, 1862, I published in the *Electrician* the results of my experiments on what is known as the "coil current" or "false discharge." I then showed that if a short length of coiled gutta percha covered wire was connected, as in diagram marked No. 3, in which C is the coil, F the battery, G the galvanometer, and L the key, the discharge would be much in excess of the charge, and of an opposite character to that of the true discharge when the distant end was insulated, and the difference was much more marked when the wire was wound on an iron reel.

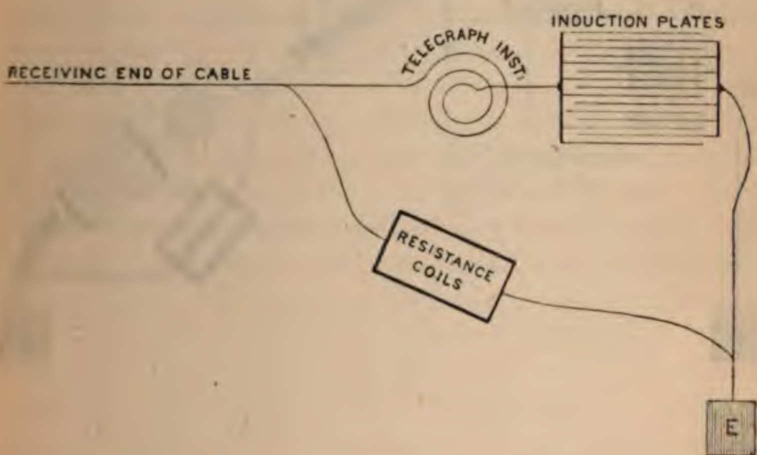
3



In the December following, Mr. Varley patented five different modes of working long subterranean or submarine lines by the aid of induction plates, induction coils, resistance coils, and an

ingenious adaptation of the effects of the "false discharge." The five modes are shown in diagrams Nos. 4, 5, 6, 7, and 8.

4



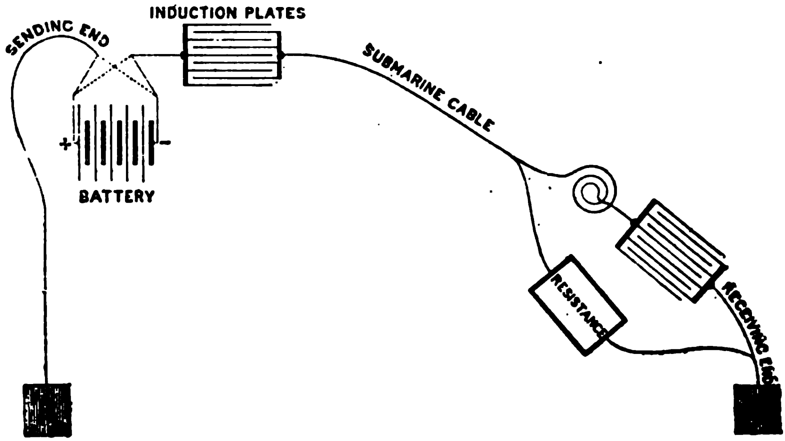
Referring to diagram No. 4, Mr. Varley says that the resistance coil would be more effective if wound on a large iron core, and that without such a resistance coil the condenser or induction plates would be quite useless.

The arrangements shown in diagram No. 5 are the same as those in diagram No. 4 at the receiving station: but at the sending station, induction plates are to be placed between the cable and battery.

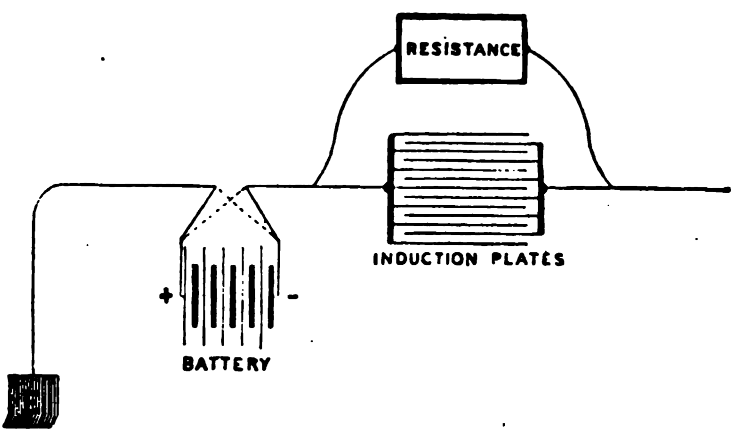
Diagram No. 6 shows the same arrangement, with a resistance coil connected as a shunt to the induction plates at the sending station.

In the arrangements shown in diagram No. 7 the resistance or induction coil is connected to the conductor and earth at the sending station; and in the arrangement shown in diagram No. 8 there is a combination of the induction plates, resistance coil, and induction coil at the sending end, with the addition of an induction coil at the receiving end.

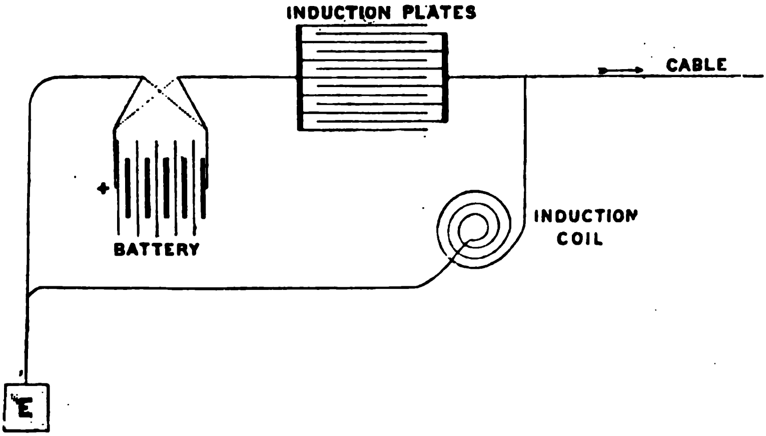
5


SENDING END
INDUCTION PLATES
SUBMARINE CABLE
+
-
BATTERY
RESISTANCE
RECEIVING

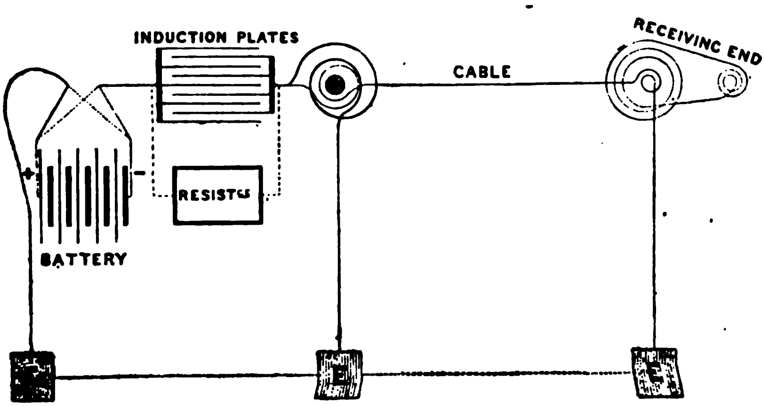
6


RESISTANCE
INDUCTION PLATES
+
-
BATTERY

7


INDUCTION PLATES
CABLE
+
BATTERY
INDUCTION
COIL
E

8


INDUCTION PLATES
CABLE
RECEIVING END
+
-
RESIST
BATTERY
E
E

I am not aware that any one of these various systems of Mr. Varley was ever practically applied to either a submarine or subterranean line; but if so, I imagine the results were not satisfactory, for on the 6th July, 1865, just before the starting of the expedition to lay the cable across the Atlantic between Valentia and Newfoundland, Messrs. Thomson and Varley conjointly patented "Improvements in Electric Telegraphs," Mr. Varley being electrician to the Atlantic Telegraph Co., and Sir William Thomson scientific adviser to the same Company.

The improvements consisted of what was designated the "Curb Key;" a mechanical arrangement by means of which three or more currents of equal strength, but of unequal duration, could, in quick succession, be alternately sent into the conductor. By merely pressing an ordinary key the current to give the signal at the distant end was set up, and at the same time a spindle was set rotating, on which were fixed a series of cams, which in quick succession came into contact with the conductor of the cable, and charged it alternately with positive and negative currents. The cam currents had to be so adjusted as to be of insufficient strength to give a signal at the distant end, but were to be regulated so as to neutralise each other, and thus discharge the core for the next signalling current. From actual experiments the patentees had found that from five to seven currents would be practically required to work the Atlantic cable. This was the recognised system to be used by the Atlantic Telegraph Co. to work their cable when laid.

On the same day as the date of the patent a party of gentlemen, at the invitation of the Atlantic Telegraph Co., assembled on
the *Great Eastern*, then lying at the Nore, to witness the
of the "curb key" through 2,273 nautical miles of cable
three tanks on board, but joined in one
end of the conductor was attached
mirror reflecting galvanometer in
and the other terminal of the ga
This was designated the recei
conductor of the cable was
apparatus, or "curb key."
nated the sending statio

station, and Sir William Thomson, assisted by Messrs. Carson and Tripp, read at the receiving station. The Morse code was used. With a positive current the beam of light was deflected to the left on the scale of the receiving galvanometer, which represented the dot, and with the negative current, the beam of light was deflected to the right, which represented the dash. A disputed speed of six words per minute was with difficulty received while sending the test message.

On July 15th the Expedition started to lay the cable, which prevented further speed experiments being made, and as the laying of the cable was not completed that year, the "curb key" was not practically employed.

Before starting on the Expedition in the following year to lay a new cable and also to complete the one which had been partly laid the previous year, I had matured my plans for a new system of testing and speaking while submerging long submarine cables; the idea of which had suggested itself to me while on board the *Great Eastern* on the previous expedition. Hitherto it had been the practice to devote certain portions of each hour to the different electrical tests which it had been considered necessary to make; for instance, during the laying of the portion of the Atlantic cable in 1865, the hour was divided into four parts, and a certain test had to be repeated every hour at the time allotted to it. In this instance the first thirty minutes were occupied on shipboard in testing the insulation, or resistance of the gutta percha, of the entire cable, and during that time the end of the conductor on shore had to be isolated, or insulated, as it is termed. The other thirty minutes were divided into three equal parts, which were devoted respectively to receiving reversals from the shore, ascertaining the resistance of the conductor, and sending reversals to the shore. The most important tests, and the only tests which are absolutely necessary during the submerging of a cable, are those for insulation and for the continuity of the conductor. A ready means of immediate communication between the ship and the shore is also desirable. Hitherto each test had been applied independently; that is to say, while the continuity test was being made, the insulation test had to be neglected, and *vice versâ*.

Consequently, in the arrangements above mentioned the state of the insulation of the cable was not known during the last thirty minutes of each hour, so that if a fault passed overboard during that time, it might be two or three miles astern before it was known to exist.

Now, in the system which I introduced, my object was to keep up a constant test for insulation and continuity, and at the same time to communicate freely through the cable between the ship and the shore.

I will here remark, in order to assist in making the system more readily understood, that were it possible for a submarine cable and the water surrounding it to be placed in an insulated trough, the resistance of the gutta percha enveloping the conductor could be ascertained, not only by measuring the amount of current passing into the cable after a certain time had been allowed for electrification, which constitutes the ordinary insulation test, but also by merely placing a wire from one terminal of a galvanometer in connection with the water, and a similar wire in connection with the other terminal to earth, the actual amount of current passing through the dielectric could be measured, and thus tests could be made by two separate persons quite independently of each other.

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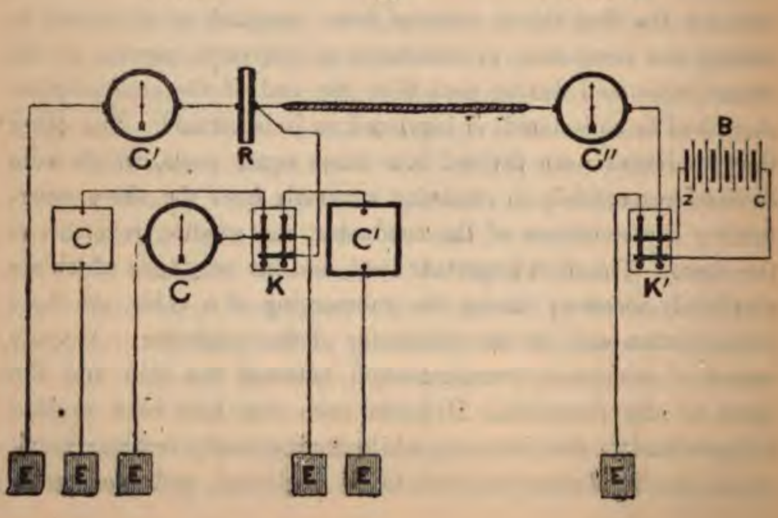

Although such an arrangement as that just described is impracticable as regards the entire cable, there would be no difficulty in placing a few miles of the core of the cable of the shore end under the conditions explained, and that would have been sufficient for my purpose. But I attained my object in a much more convenient and cheaper way. I placed a series of sheets of tinfoil and gelatine alternately one on the other, and each alternate sheet of tinfoil I connected to the shore end of the conductor of the cable, and the other sheets of the metal were connected to the terminal of a very sensitive astatic mirror galvanometer, the other terminal of the galvanometer being connected to earth as shown at R in diagram 9.

Consequently the current which passed through the sheets of gelatine flowed through the galvanometer to earth, and a constant deflection was registered so long as the cable was kept at a uniform tension. But if the tension were altered or the direction of the current reversed, the change of the deflection was immediately noticed by the person in charge of the galvanometer on shore, and by this means the person in charge of the batteries on shipboard could by a pre-arranged code of signals communicate with the person in charge of the galvanometer on shore, without interfering with the insulation test. The portion of the current passing through the gelatine plates, R, was only about 0·0025 of the whole charge in the cable, which was hardly sufficient to increase the deflection on the ship galvanometer, but nevertheless amply sufficient to give a deflection of two hundred divisions on the scale of the sensitive shore galvanometer.

To enable the person in charge of the distant galvanometer (shore station) to communicate the continuity or speaking signal to the battery station (ship), I placed two resistance coils of unequal resistance, as shown at C and C′ in diagram No. 9, so that when one spring of the key K was depressed it allowed a minute portion of the charge in the cable to pass to earth, and thus produce on the ship galvanometer an increased deflection of a few divisions, which was sufficient for the continuity signal, or to represent the dot when speaking. If the other spring of the key were depressed, it would allow double the amount of current to pass to earth, thus increasing the deflection twofold on the ship

galvanometer, which, when speaking, would represent the dash, assuming the Morse code to be used.

As soon as I had perfected the whole system, I engaged a room in a herdsman's cottage on the shore of the Medway nearly opposite to where the *Great Eastern* was moored. This I fitted as the shore station, and laid an insulated wire from it to one end of the core of the cable on board the *Great Eastern*, and employed the electrical staff who were to be engaged while the cable was being laid to work the same system through the entire length of the cable while it was being coiled on board the *Great Eastern*. Thus, not only were continuous tests applied during the coiling of the cable, but the operators got well used to their respective duties before the actual laying of the cable commenced.

Early in February, 1866, a party of gentlemen were requested to examine and report on the practicability of the system; and Mr. Latimer Clark in his report stated:

"I have examined the system carefully, and have experimented upon it through upwards of a thousand miles of the Atlantic cable now on board the *Great Eastern*, and I find it in every way suited for practical use; it allows of the free application of the various tests requisite during submergence of a long submarine cable, and it has the peculiar merit of giving an absolutely continuous test of insulation, and at the same time of enabling the tests for continuity of the conductor, and the transmission of intelligence, to be carried on uninterruptedly without the necessity for any change in the connections of the apparatus employed either on ship or on shore."

Sir William Thomson in his report stated:

"I found its action through the cable on board the *Great Eastern* quite perfect so far as signalling each way without interfering with the ship's insulation test is concerned. The plan by which he allows the shore operator to signal to the ship is particularly simple and well arranged."

After the completion of the day's proceedings I received most hearty congratulations from Sir Daniel Gooch, Sir Fothergill Cooke, and other gentlemen who were present, and had taken great interest in all the experiments. The system continued to work most satisfactorily, and the operators soon became proficient

in their work; but my friend Mr. Laws pointed out that if through carelessness or by accident either of the resistance coils should be left in contact with the conductor, by a continued depression of the spring or springs of the key K, in diagram No. 9, the permanent increased deflection on the ship's galvanometer might be mistaken for an incipient fault having occurred in the cable, and thus create confusion. He suggested that the resistance coils should be replaced by condensers of unequal capacity, which would, when charged separately from the cable, give about the same deflections on the ship galvanometer as the resistance coils had done, and by discharging one of them after it had been charged a definite time to give the continuity signal, through a galvanometer, it would give the potential of the cable at the time, and be a check on the galvanometer G′, diagram No. 9, and these data carefully recorded might prove serviceable, should a small fault occur, in assisting to localise the same. Although nothing but the most palpable carelessness could have produced the result pointed out by Mr. Laws, I saw an advantage in his suggestion, and at once adopted it. Diagram No. 9 shows the whole arrangement I proposed to adopt during the time occupied in submerging the cable. R represents the combination of sheets of tinfoil and gelatine; G′ a very sensitive astatic mirror reflecting galvanometer, so adjusted as to show a deflection of about two hundred divisions from the current passing through the gelatine to earth, while the cable was kept charged from one hundred Menotti cells. Assuming the battery power used to equal one hundred volts, then the current producing the two hundred divisions would equal 12·4 millionths of the whole, or two hundred and fifty microvolts. This would enable ship to communicate to shore by reversals, reduced or increased tension. C and C′, two condensers of unequal capacity, to enable shore to give the continuity signal to ship, and also to communicate by the Morse code when necessary, C′ representing the dash and C the dot. G a similar galvanometer to G′, and so connected through key K, that after C′ had been charged to give the continuity signal, the discharge would pass through it, and thus be a check on G for the potential of the cable. G″ an ordinary marine reflecting galvanometer, K′ the ordinary reversing key, and B the battery. This

simple arrangement enabled ship and shore to note the insulation and continuity of the cable and to communicate with each other when necessary.

Before starting to lay the cable, my plans had to be finally submitted to Messrs. Thomson and Varley, as representing the Atlantic Telegraph Co., and the latter gentleman suggested that instead of using two condensers for speaking, only one should be used, and that instead of charging it from the cable at stated intervals, it should be permanently attached to the conductor of the cable, and when required to speak to ship, reversals from a local battery should be sent into the condenser, which would either increase or diminish the deflection on the ship galvanometer by induction; and to enable ship to speak to shore, the local battery should be replaced by a galvanometer, so that by sending reversals from a separate battery into the already charged cable, the ship would either increase or diminish its potential, which would act inductively on the condenser, and cause deflections either to the right or left on the shore galvanometer. Mr. Varley's suggestion had the advantage over that of Mr. Laws, inasmuch as the signals were sharper and more distinct, and did not cause the beam of light to wander so far on the scale of the galvanometer.

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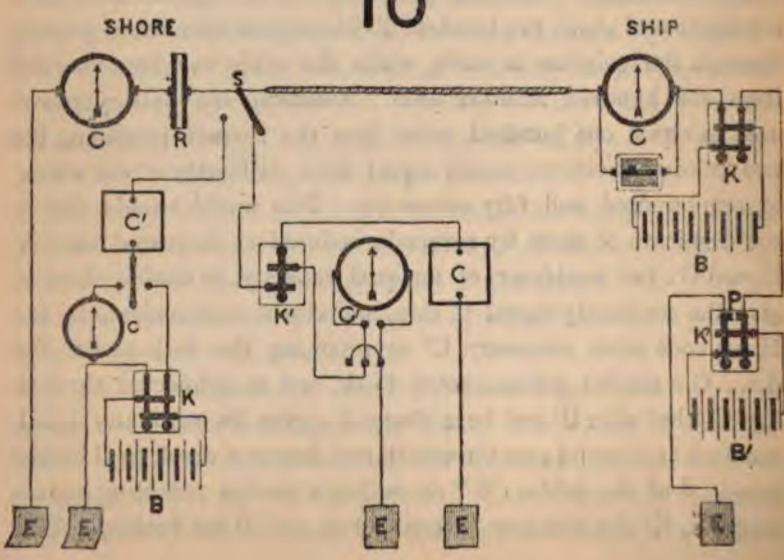

It was therefore agreed that the plan of using a single condenser with a battery was preferable to that of the two condensers without the additional batteries. Consequently the system actually employed during the laying of the 1866 and the completion of the 1865 cables was as shown in diagram No. 10, which it will be observed is similar to that shown in diagram No. 9, with the exception that only one condenser, C′, and the battery B were used for speaking, while the other condenser, C, was employed to give the continuity signal, and the discharge used as a check on G to ascertain the potential of the cable at the shore end. The extra battery, B′, on shipboard was also employed while speaking to the shore, by means of the key, K′, after removing the plug P.

The whole system worked remarkably well. During the fourteen days occupied in laying the cable from Ireland to Newfoundland, 164 messages in all were sent and received between the ship and shore, containing 6,437 words, or 30,059 letters. Of course speed was not the object while speaking, as it had to be arranged so as to interfere as little as possible with the regular tests. It was, however, soon apparent that a higher speed would be obtained by the use of a condenser than by the "curb key"; consequently, on the completion of the laying of the cable, the

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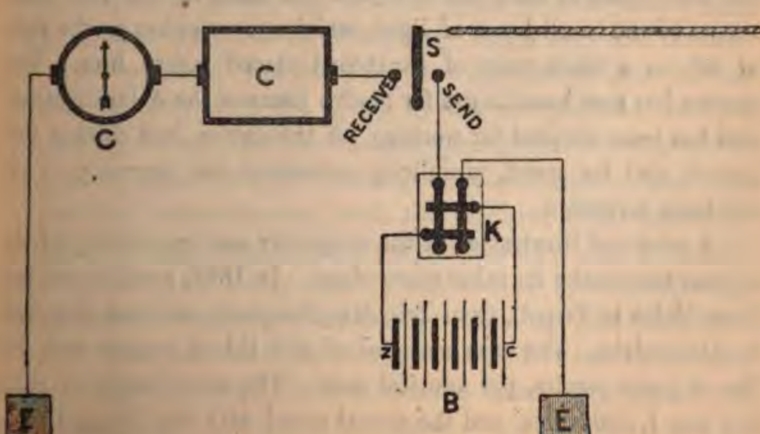

paying out system was continued, and before leaving Heart's Content I had the gratification of seeing a speed of thirteen words per minute attained with perfect ease.

Subsequently, on my return home, I altered the arrangements, as shown in diagram No. 11; in which C shows the condenser, the electrostatic capacity of which was equal to 75 miles of the cable, G receiving galvanometer, S switch, K ordinary reversing key, and B battery of ten Menotti cells. The arrangements were the same at each end of the cable. When not working, the switch S at each station was turned to the contact marked "receive," so that each end of the conductor would be connected to a condenser. The operator at either station wishing to speak would "switch over" to the stud marked "send," and commence sending reversals by the key K, to represent the usual call signal; then "switch over" and wait until he received the reply to his call, when he would again "switch over," and send what he had to communicate. With this simple arrangement, I have seen 25 words per minute received at Valentia from Heart's Content through 1,900 nautical miles of cable, with the most perfect ease. If we consider for a moment what 25 words per minute really mean, we shall be better able to realise the credit due to those gentlemen who are, what is termed, "good mirror clerks." For a speed of 25 words per minute, the sender has to manipulate the springs of the key K, so as to make 375 contacts per minute; and the receiver has to distinguish, in the same space of time, the true from the false, in the 750 movements of the small beam of light, which moves either to the right or left on a white strip of cardboard placed before him. This system has now been in use for twelve years on the Atlantic cables, and has been adopted for working all the cables laid during that period, and for speed, simplicity, economy, and accuracy, it has not been surpassed.

A practical illustration of the simplicity and superiority of the system may make its value more clear. In 1861, a cable was laid from Malta to Tripoli, from Tripoli to Benghazi, and from Benghazi to Alexandria. The core consisted of 400 lbs. of copper and 400 lbs. of gutta percha, per nautical mile. The total length of cable laid was 1,330 miles, and the actual speed with the Morse instru-

ment, which was the instrument employed, was 3·2 words per minute. The only way known to increase that speed was to work each section separately. On my return from the Atlantic expedition in 1866, I was requested to proceed to Malta, and apply the system which had proved so successful on the Atlantic cables to the Malta and Alexandria cable. I therefore instructed my assistant, Mr. May, who had so ably discharged the duties entrusted to him while in charge of the shore station at Valentia during the laying of the Atlantic cables, to proceed to Alexandria, and at a certain time, which was agreed upon, to connect to the end of the cable there the arrangement as shown in diagram No. 11. But in this case C consisted of alternate sheets of tinfoil and gelatine, similar to R in diagram No. 9, hurriedly put together. I proceeded to Malta with similar apparatus. On my arrival there, I learned that they had not been able to communicate between Benghazi and Alexandria for some days, owing to a fault of very low resistance in that section of the cable. The repairing ship had left Malta, and was supposed to be at work endeavouring to remove the fault. But as no alteration had taken place within a few hours of the time which I had arranged to speak with Mr. May, I requested Tripoli and Benghazi to join the cable through for a definite time at their respective stations; and having connected the instruments as shown in diagram No. 11, with the switch over to "send," at my end of the cable, at the appointed time, the call signal was sent at Malta, and, to the surprise of some and delight of all, prompt as an echo came the reply, quite clear and distinct. Now the operators both at Malta and Alexandria had had no practice whatever with the mirror system of signalling, and yet they worked so well that the line was opened the following morning for the despatch of public messages. Unfortunately there were no means of communicating with the repairing ship; and thus, after the cable had been working very successfully, both financially and electrically, for seven days, it was broken by the repairing ship in endeavouring to get at the fault. Had the cable not been broken, it is impossible to say how long it would have been worked, but most probably until, at least, a more favourable time of the year for repairs.

The greater part of this cable was laid along the coast in shallow water, and consequently was frequently getting more or less damaged. In 1868, this cable was duplicated by laying a new cable direct between Malta and Alexandria in deep water. The proportions of the gutta percha and copper in the core of this cable were about one-third of those in the cable laid in 1861. I applied the Atlantic system, as shown in diagram No. 11, to this cable as soon as laid, and the speed of the first message was nineteen words per minute, and I believe it is working as satisfactorily at the present time.

The only objection I have ever heard against the system is, that the mirror galvanometer is not a recording instrument, and hence the introduction by Sir William Thomson of his very ingenious but complicated and expensive instrument, the "Syphon Recorder," which he patented in 1867. The recorder was simply intended to replace the galvanometer as the receiving instrument, the other parts of the original system to remain intact. The principle of this instrument is shown in diagram No. 12.

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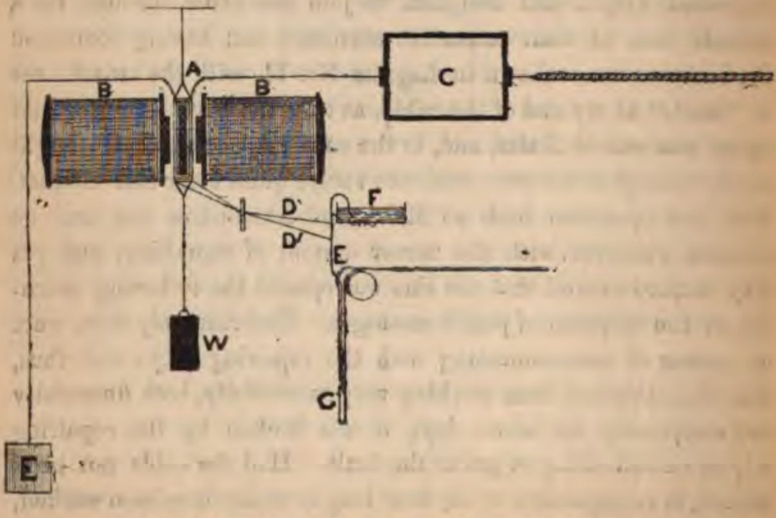

A is a coil of fine insulated wire, delicately suspended between the poles of a very powerful electro-magnet, B B. The current

which produces the signal passes through the coil, which causes the coil to take up a certain position relatively to the poles of the magnet. If the current is positive, the coil turns, say, to the left, but if the current is negative, the coil turns in the opposite direction. W is a weight, so arranged that it not only keeps the coil in position, but also prevents it from turning too far, and serves to bring the coil back to its original position when no current is passing. D and D′ are two silk fibres, attached as shown to the coil A, and a very fine glass syphon E, one end of which dips into a small pan containing ink, while the other is free to move across the paper ribbon G in obedience to the movements of the coil A. The paper moves past the end of the syphon at a uniform speed. In order to make the ink pass through such a fine tube as the syphon E, it is electrified, and the paper is connected to earth by means of the reel over which it passes; consequently, the ink is ejected on to the paper in quick succession, representing a continuous line of minute points along the centre of the paper, so long as no current passes through the coil A; but when this coil is deflected by the passage of a current through it, the end of the syphon is deflected by means of the silk fibre D or D′, and marks a wave line on the paper, showing the successive deflections to the right or left of the central position, and these deflections represent the dot or dash of the Morse code. C is a combination of induction plates, the same as shown in diagram No. 11. With the completed instrument there is an electrostatic induction machine, for the electrification of the ink, which is driven by an electro-magnetic machine, which also works the mechanism for drawing the paper past the end of the syphon. A local battery to excite the electro-magnets B B and the electro-magnetic machine, and resistance coils, which are used as shunts to regulate the speed of the machine by increasing or decreasing the force of the current, are also used.

I have had no experience of the actual working of the syphon recorder in connection with laid cables, but I have endeavoured to learn all I possibly could from those who have; and I find there is a great diversity of opinion as to the relative merits of

the two instruments. It appears to me that a *good* mirror clerk prefers the galvanometer, while an *inferior* reader of the mirror prefers the recorder. I have, however, learned nothing to cause me to alter my opinion already expressed, that for economy, simplicity, accuracy, and speed, the mirror is the superior instrument.

In 1873, Sir William Thomson and Professor Jenkin conjointly patented what they termed "The Automatic Curb Sender," which appears to me to be a combination of the "curb key," patented by Sir William Thomson and Mr. C. F. Varley conjointly in 1865, and Sir Charles Wheatstone's well-known "automatic sender," which he patented in 1859. The working capacity of long cables is no doubt limited to the dexterity of the fingers of the sender, and the quickness of the eye of the receiver; therefore such an instrument, to my mind, seems to be a step in the right direction, but it will be of little value until a suitable receiving instrument is discovered to work with it.

I will now direct your attention once more to diagram No. 4. In Mr. Varley's patent, to which that diagram refers, he says that without the resistance coil connected as shown, no advantage would be obtained in using a condenser. Now in the present

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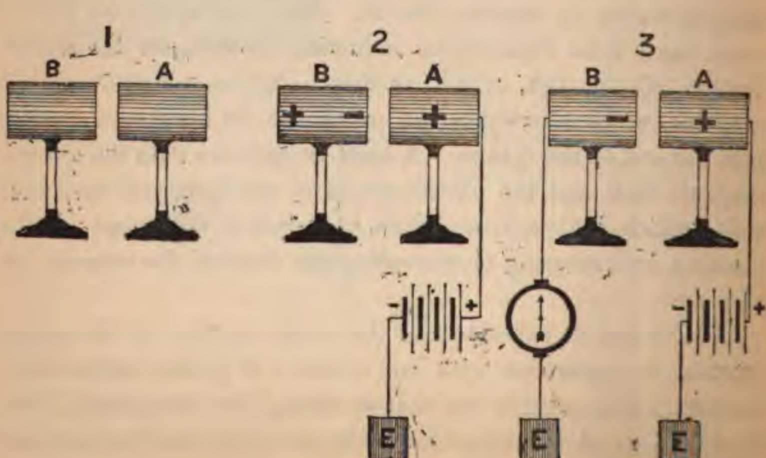

system of working long cables, it is quite the reverse, the resistance coils, if used, whether wound on an iron core or not, would simply act as shunts, and would consequently reduce the speed in proportion to their resistance. This will be readily understood if we consider for a moment the electrical law by which the system is worked. Two insulated metal cylinders, marked respectively A and B, placed near each other as shown in fig. 1, diagram No. 13, will show no sign of being charged with either positive or negative electricity; but if A be connected with a battery, as shown in fig. 2, then B will be immediately affected by induction, and be found to contain both positive and negative electricity in a free state, their relative positions being as marked. If a galvanometer be connected, as shown in fig. 3, the electricity of the same character as that in A will pass to earth, and the electricity of the opposite character to that in A will remain, being held captive, as it were, under the influence of the charge in A. But the slightest variation in the potential of A destroys the equilibrium in B; and thus by increasing, decreasing, or reversing the charge in A, it would cause a current to pass through the galvanometer to earth or *vice versâ*. The cylinders placed as shown in fig. 3 constitute, to all intents and purposes, what is called a condenser, or better still to my mind, induction plates; and if it be assumed that the connection between the battery and the cylinder A is the conductor of the cable, it fairly represents how the signals are produced at the receiving station.

Assuming the current from ten Menotti cells to be sent into the cable on each contact at the sending station, and that would be more than sufficient for a cable 2,000 miles in length, if worked on the mirror system, then the amount of induced current passing through the receiving galvanometer would be equal to about 1.40,000th part of that current; or, in other words, supposing the power used to be ten volts, the power which gives the signal would be equal to two hundred and fifty microvolts. Let the line A and B in diagram No. 14, represent the conductor, and the lines C D the boundary of the potential of the charge on immediate contact; then the minute portion at A encompassed by the two perpendicular lines 1 and 2, will represent the actual amount of the charge which

the keeper from the magnet, or varying the resistance of the resistance coil, the true or false discharge predominated. From the results of these and similar experiments, I was under the impression that by a judicious distribution of electro-magnets on subterranean lines greater speed might be obtained.

Now the syphon recorder is a most valuable instrument for this class of experiment, as it actually draws on paper the force and nature of the current affecting it. I therefore used a recorder as the receiving instrument, and made a series of experiments, but in every instance the addition of the electro-magnets had a prejudicial effect. Diagram No. 16 contains fac-similes of the comparative signals obtained under the conditions last described, with the exception that the recorder was in circuit as the receiving instrument.

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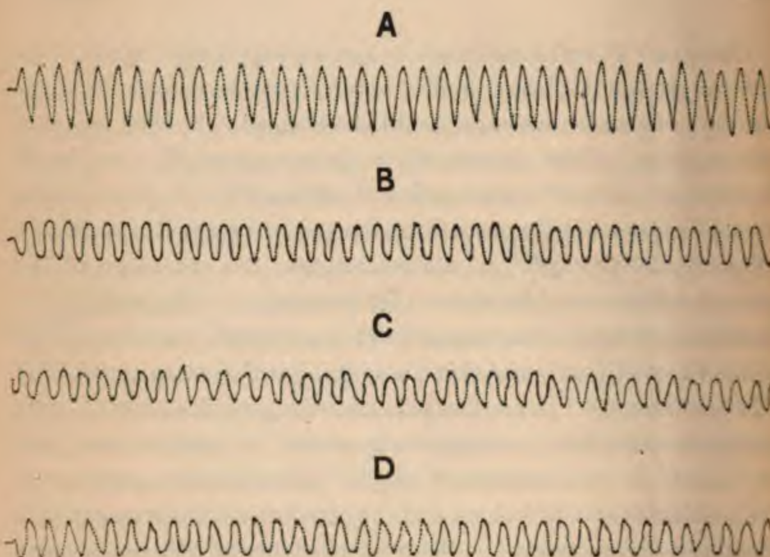
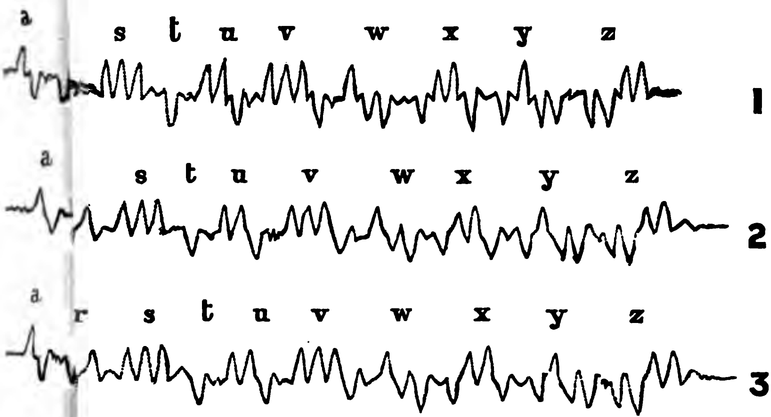
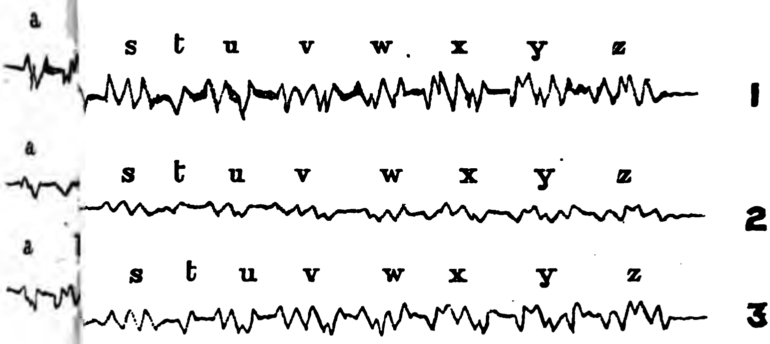


The speed was 220 reversals per minute from ten Menotti cells. A shows the signals received through a resistance equal to that of the copper conductor of the core. B shows the signals received



a s t u v w x y z 1
a s t u v w x y z 2
a r s t u v w x y z 3



a s t u v w x y z 1
a s t u v w x y z 2
a s t u v w x y z 3

through the eighty-five knots of core, and if compared with A, very prettily illustrates the effect of induction, even in so short a length of submerged wire. C shows the signals when the electro-magnet was attached, and D, when the additional resistance was added. The experiments were continued with varying speeds and battery power, and electro-magnets placed in every conceivable way; but the results were in every instance unfavourable to the employment of electro-magnets as shunts, as suggested by Mr. Varley.

Fig. 1, diagram No. 17, is a *fac-simile* of the Morse alphabet received by the recorder through a resistance of 3,046 ohms from six Menotti cells, connected as shown in diagram No. 11. Fig. 2 shows the same signals received under the same conditions as in fig. 1, with the exception that the resistance coil of 3,046 ohms was replaced by 255·72 knots of submerged core, the resistance of the conductor of which was equal to the previous resistance, and its inductive capacity 84·5 microfarads. Fig. 3 shows the same received through the same length of cable, but in this case the conditions at the sending end were as shown in diagram No. 18; and the batteries proportioned as marked were found to give the best results for that length of cable.

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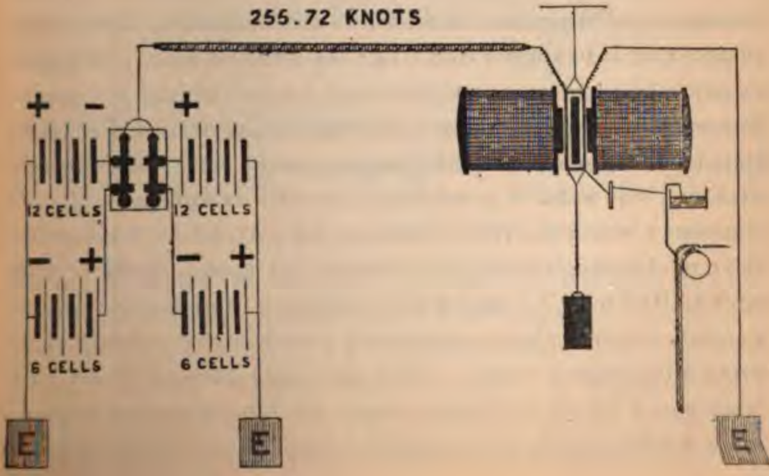

With a condenser of a capacity of twenty microfarads placed between the receiving end of the cable and the recorder, as shown in diagram No. 12, the results were as shown by the figs. 1, 2, and 3 in diagram No. 19. These figs. are comparable with the corresponding figs. in diagram 17, and if the signals marked 2 in the respective diagrams be compared, it will be seen to what extent the amplitude of the curve is lessened by the introduction of the condenser. From the results of these experiments it would appear that an advantage would be gained, if instead of putting the sending end of the conductor to earth after each contact with the battery, as is the ordinary way of working long submarine cables, it was brought into contact with a pole of a battery of the opposite sign to the previous contact, as shown in diagram No. 18. If figs. 2 and 3 in diagram No. 19 be compared, the advantage becomes at once evident.

Within the last few years, most of the long submarine cables have had the duplex system of working successfully applied to them. On entering on this part of my subject, I cannot refrain from remarking how often the experimentalist has at his command, unknown to himself, the germ of some great discovery, but which passes by him unnoticed. When early in 1752, Mr. Kennersley, of Boston, informed his friend Franklin "that he placed the needle of a compass on the point of a long pin, and holding it in the atmosphere of the prime conductor at the distance of about three inches, found it to whirl round like the flyers of a jack, with great rapidity," he had no conception that he had within his grasp a discovery which sixty years afterwards immortalised Professor Oersted. Or when in 1756 it was suggested to the same gentleman that "as water is a conductor as well as metals, it is to be considered whether a river or lake or sea may not be made part of the circuit through which the electric fire passes, instead of the circuit all of wire," I expect this gentleman little thought that his suggestion would play so prominent a part in the economy of our present telegraphic system. The same may be said of Mr. C. F. Varley and Mr. C. V. de Sauty, when the former suggested the use of an artificial cable, as an adjunct, to facilitate the working of a

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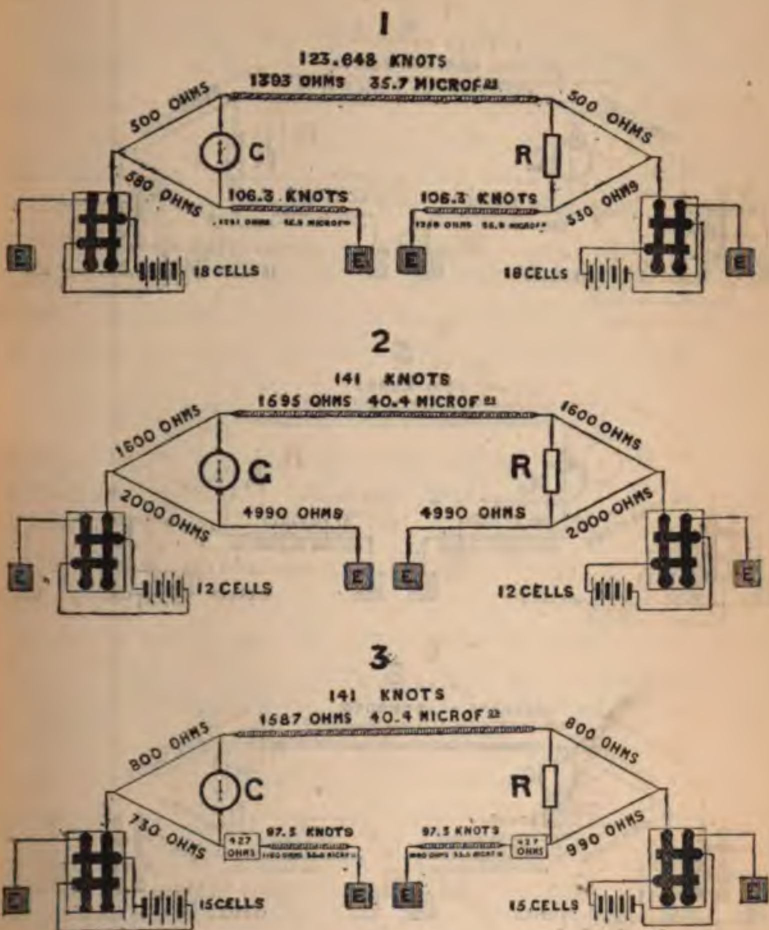

laid cable; and the latter when he introduced the bridge system with condensers for balancing, and thus measuring, the electrostatic capacity of cables: for I suppose neither of these gentlemen had, at the time, any idea that, with slight modifications, their respective suggestions would play such an important part in the present system for *duplexing the working* of submarine cables.

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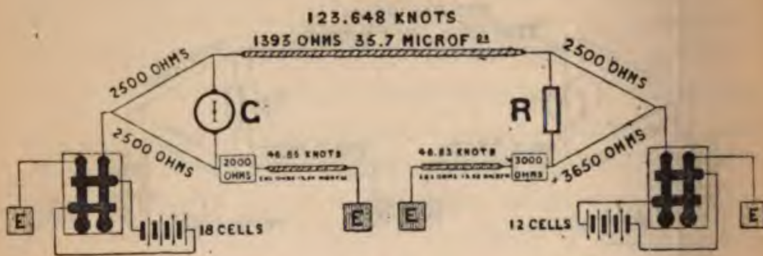

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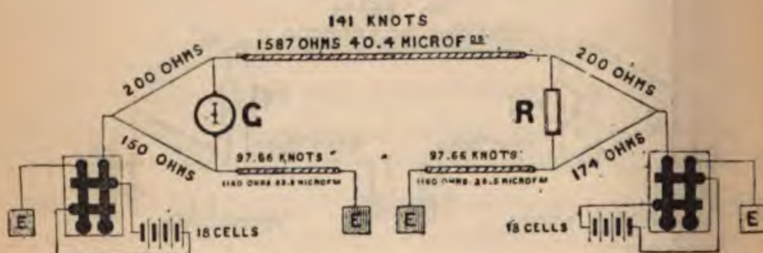
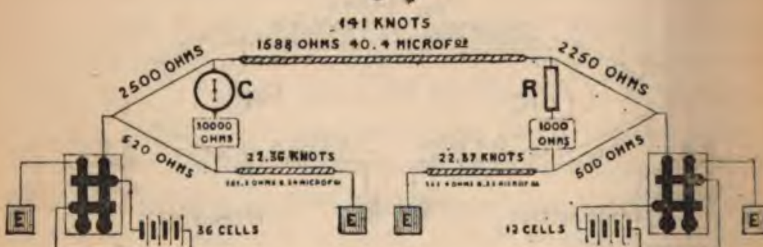
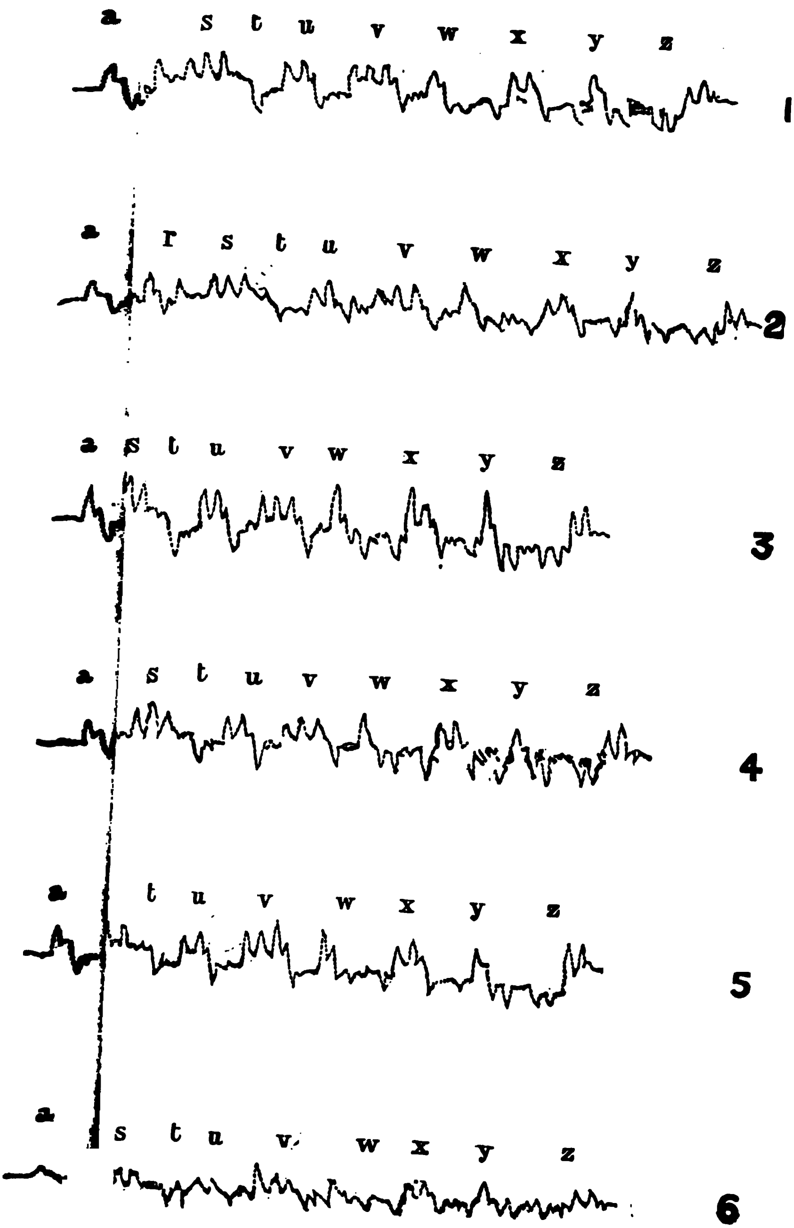


6



In duplex working, the principle remains the same as described with diagram No. 13; but the receiving instrument at each station is so placed, that the current sent to signal the distant station does not affect it. To insure good duplex working, a true balance must be established at the sending end, so that no current will pass through the receiving instrument when placed as in fig. 1, diagram


a s t u v w x y z
1
a r s t u v w x y z
2
a s t u v w x y z
3
a s t u v w x y z
4
a t u v w x y z
5
a s t u v w x y z
6

No. 20. The figs. marked respectively 1, 2, 3, 4, 5, and 6 in this diagram represent the several conditions under which a good balance was obtained in actual experiments. In fig. 1, it will be seen that the main line was 123·648 knots in length, the resistance of the conductor of which was 1,393 ohms, and its absolute inductive capacity 35·7 microfarads. This was balanced at the end of the cable, which had the recorder for a receiving instrument, by 106·3 knots of core, the resistance of the conductor of which was 1,256 ohms, and its inductive capacity 35·9 microfarads, the resistance of the respective sides of the fork being 500 and 530 ohms. At the other end, where a signalling galvanometer was used as the receiving instrument, the balance was similarly established. Diagram No. 21 contains *fac-similes* of the alphabet received on the syphon recorder slips, answering respectively to the six modes of balancing for duplex working shown in diagram 20. As the amount of the resistance and inductive capacity are given with each experiment, and also the character of the signals obtained, I need not enumerate them here. In fig. 2 it will be observed that the balance is obtained by resistance only; but the signals are not so good as in fig. 1, in which inductive capacity was also used. The best results were obtained under the conditions given in fig. 3. In fig. 4 it will be observed, that the inductive capacity at each end is only half that of the cable, but the resistance is much higher. In the experiments shown I have used similar core to that of the cable to obtain the necessary inductive capacity, but of course condensers or induction plates might have been employed with equal effect.

Wonderful indeed is the progress which has been made in electrical science during the last few years; but I believe we are only upon the threshold of what we may be permitted to know, and that there are before us gleams of light, which, if rightly pursued, will richly reward the patient and honest explorer.

The PRESIDENT: We have just listened to a very interesting paper by an exceedingly practical man. I think the subject is a very interesting one, very important to us, and no doubt many gentlemen will have observations to make upon the question

treated. I will in the first place call upon Mr. Varley to open the discussion.

Mr. C. F. Varley: I wish, sir, you had called upon some other person, as not having had a copy of the paper, and being unfortunately somewhat deaf, and as the acoustic properties of this room are not all that could be desired, I have not been able to follow Mr. Willoughby Smith's paper, but if the old custom had been kept up of supplying members with copies of papers before they are read, especially such a paper as this, I should have been prepared to discuss it. I shall, however, have great pleasure in making a few remarks now, if I am allowed on a future occasion to supplement them with a few statistics and some further remarks, which it is only fair to the subject and to myself should be laid before the Society. Upon such conditions I am willing to commence the discussion.

The President: I may say another paper has been put forward this evening, treating on a subject very much akin to that which you have heard from Mr. Willoughby Smith. It is a paper on "Curbed Signals for Long Circuits," by Mr. James Graves, one of our Members. I think, perhaps, it would be more convenient that the paper should be read now, as there is some difficulty in arranging how papers can be read, and we have limited opportunities of reading them. I would suggest, therefore, that Mr Graves' paper should be read, and then the discussion can take place on both papers together.

The Secretary accordingly read the paper

ON CURBED SIGNALS FOR LONG CABLES.

By James Graves, *Member*.

The commercial value of long cables being so dependent upon the speed with which signals, letters, and words can be legibly transmitted through them, it is only natural that various schemes should be devised, by as many different persons, for increasing the speed of reception, by improving the definition of the signals at rapid rates of transmission, whether manually or automatically.

In the early days of telegraphy it was soon discovered that *long land circuits* which included subterranean wires, and sub-

marine cables connecting England with the continent of Europe and the sister island, were advantageously worked by means of reversed currents, sent from double-current keys, and the general idea seems to have continued in a great measure, up to the present time, that all submarine cables ought to be worked with advantage upon the same system.

But in all the elaborate devices which have been contrived for this purpose, the most vital point appears to be lost sight of, that, in the old subterranean and submarine lines, the dot and dash alphabet was almost, if not quite, always used for the purpose of signalling; in which the marking current was of one fixed polarity, while the spacing current was of the contrary polarity; if the marking current was positive the spacing current was negative, and whether dots or dashes were transmitted every signal was formed of alternate positive and negative currents, and neither two positives nor two negatives ever succeeded each other.

By such a system of signalling, it is quite clear the definition of the letters would be improved.

Various forms of double current keys have been in use from the days of Varley's "zinc sender" to those of his wheel-keys, and the more modern pattern of double current key, all of which were designed for carrying out the same principle—that of sending a reversed current after each marking current to clear the line for the next signal; the advantage of which will not, for a moment, be doubted.

When, however, we come to long ocean cables, worked, nevertheless, by the same alphabet, but signalled in an entirely different manner, and instead of dots and dashes of the same polarity but of unequal lengths, we use rights and lefts on the mirror, or ups and downs on the siphon-recorder, of equal lengths but of opposite polarities, the conditions are entirely changed; all the old notions of reversals to clear the line fall to the ground, and a new field opens to our view, because the former regular succession of alternate positives and negatives no longer applies to the conditions of signalling.

Taking the alphabet as it stands (and which is too well known to be here repeated) it will be seen at once that the only letters

fulfilling the conditions of the old system are e, i, s, h, t, m, o, ch, being only one-third of the whole alphabet, and these would undoubtedly be improved in definition by a reversal being sent after each marking current in regular alternations, or by "curbing" as this is technically called.

In the remaining two-thirds of the alphabet each letter is composed of a mixture of dots and dashes, and where the reversal takes place from dot to dash, or dash to dot, the curbing current appears to be too much, and does not improve the definition of the first signal after the reversal, but rather the contrary, *e.g.*, the letter *a* (- —) assuming + for the dot, and — for the dash, on the curbing system the order of the currents would be + —, — +. The curbing current of the dot, being of the same polarity as the marking current of the dash, spoils the definition of the latter by shortening its range.

This is more clearly shown in the letter *c* (— - — -) where the curbing system sends eight currents, — +, + —, — +, + —; the effect is to shorten the first dot, then the second dash, and finally the second dot, whereas by the ordinary system of sending, the four signals would be equal in range, and this peculiarity of the curbing system is more or less visible in all letters containing reversals becoming more apparent as the speed of signalling is increased.

There is little doubt that the system of signalling now adopted on long cables—that of inserting condensers at both ends, and the use of a variable zero, have to a very great extent rendered the curbing system unnecessary, as the signals are made not by direct currents sent into the cable, but by *inductive impulses*, which can be regulated at will, and thus the maximum working speed of the cable is at once obtained by means of the ordinary cable keys and manual sending.

Sir William Thomson's Automatic Curb Sender, a most elaborate instrument, fully described in the Society's Journal, Vol. V., page 213, was designed for the purpose of ensuring strictly accurate proportions in the lengths of signal currents and spaces, and for increasing the speed of signalling by sending, automatically, a reversed current after every marking current;

but experience shows that, on long cables, the working speed cannot be increased beyond the capabilities of the ordinary cable keys.

Moreover, the supply of traffic is such that, with the present cable capacity, there is never such an accumulation of work as to necessitate punching and automatic sending, as this process would occupy as much time as sending the work by hand; but if at any future time the whole of the Atlantic traffic should have to be concentrated for transmission upon a single cable, some advantage might be derived from the employment of the automatic system of sending, as there would be ample staff to punch the slips as the work accumulated, and by sending batches of say 50 messages each way alternately at the maximum speed of the line for legible signals, a large quantity of work could be done upon the one line, independently of the patience, temper, or exhaustion of the operators, while the mathematical accuracy of the signals and spacing would materially assist the reception. The punchers should, however, be expert cable operators, as much judgment is required in the spacing of the very difficult code and cipher words contained in many of the Atlantic telegrams, some of which would require to be doubly spaced to ensure accuracy. In manual sending the speed and spacing have to be varied continually according to the nature of the work transmitted.

The Automatic Curb Sender could easily be made to work, electrically, like the ordinary keys, by removing the curbing action, and thus attain the continuous maximum speed of sending, with signals similar to those obtained with the ordinary keys.

The signals by the automatic apparatus are regulated and sent by means of punched strips of paper. The punching instrument is a very ingenious contrivance, and is easily worked after a little practice.

Some years ago, when I had more faith in the curbing system than I now have, I designed a transmitter, which, worked by two light keys and a local battery actuating two electro-magnetic armatures, sent a reverse current before and after each marking current, the former contacts being shorter than the latter, and the battery power adjustable, but I found, in practice, after

sending a quantity of the ordinary traffic by its means, that the results were *no better* than those obtained by the simple ordinary keys which discharge the near end of the cable after each signal.

Others have worked in the same field, and under the same impression, that curbed currents are advantageous for cable signalling, with a view to mechanically provide for sending these curb-currents, but I am very strongly of opinion that, like seeking for perpetual motion, it is labour in vain.

Reasoning from the action of the ordinary keys, it is but an easy step to jump to the conclusion (perhaps somewhat hastily), that if an earth contact could be made after both the marking and spacing, or curbing currents, the definition would at once rise to perfection, and the speed be thereby increased; but when several changes have to be made in the same equivalent of time. it is evident that they cannot all be of equal duration with a less number of changes in that time, and that the greater number must be less effective than the lesser number, and fail in consequence to give that full and clear effect which is obtained from the old form of the ordinary keys, as originally adopted for working the Atlantic cables in 1866, with which each signal is made by the motion of the lever (or spring tapper) down and up, giving 50 per cent. of the time occupied as available for marking, and 50 per cent. for spacing and definition.

In the automatic the proportions are about 60 per cent. for marking of one polarity, and 40 per cent. of a reversed polarity for "curbing," as it is technically called. These proportions could, of course, be varied, by altering the relative lengths of the cam contacts, but as already shown, the system only improves letters of one polarity.

Count Emile Siccardi, of Turin, has invented and patented several varieties of curbing keys to be worked by hand in the ordinary way, but, in their present form, they are very noisy, and very laborious to work; these, however, are faults which are remediable, if the keys were electrically advantageous, but each of the various forms submitted to me for trial fails to give any increase in speed, or any improvement in definition beyond that *attainable,* with much less labour, by the ordinary keys, as the

number of contacts for each signal so reduces the effective action of each as to nullify the intended advantages of their arrangement.

This result is not only what may be arrived at by comparative reasoning, but that which is actually obtained in practice, after long and patient experiment both on the mirror and siphon recording instruments, with manual and automatic sending.

In Vol. VII., page 82, of the Society's Journal, Mr. J. Gott, of St. Pierre, after describing his own arrangement for curbing signals by means of some adjuncts to the ordinary cable key, says: "It will probably be found in practice that the usual receiving instrument does not show much improvement when the signals are curbed; this will be because the rate of vibration of the mirror, or coil, requires to be adjusted to the new condition of things."

This is an easy way of getting over the practical failure of the system, but it would have been far more interesting and satisfactory to the members of this Society if Mr. Gott had given a lucid statement of what these "new conditions" were, and of the means by which they could be rendered practically available for increasing the speed of reception upon ocean cables, instead of imputing the failure of the system to the assumed latent imperfections of the receiving apparatus, rather than to the inherent defects of the system by which the letters of the alphabet are signalled with curbed currents.

If an alphabet for cable working could be economically constructed so as to be composed wholly of signals of the same polarity, then, and then only, would the curbing system be of any advantage; and if an alphabet could be formed in which every letter consisted of reversals, then the ordinary key would be far superior to the curb key; but as neither of such alphabets could possibly be as short as the present one, the whole weight of the argument is in favour of the present mode of signalling the present alphabet with the ordinary keys, by means of which the Atlantic cables can be, and are, worked at a speed which would be considered creditable on a Morse land circuit of ordinary length, with signals which, for clearness and precision, are far superior to any that have come under my notice produced by any system of curbing, whether the

signals have been transmitted by hand, or by the Automatic Curb Sender.

Thus the ordinary cable key holds its own, at present, against all rivals, and, in my opinion, is likely to do so, setting aside the fact that special keys require special arrangements, all of which involve extra labour of one kind or another, either in the maintenance of extra battery power, in the punching of slips ready for the transmission of the traffic, or in the expenditure of much extra labour in manipulating heavy keys.

All external or additional apparatus adds, moreover, so many points where defects or failures may at any moment interfere with the smooth and rapid transmission of the traffic.

The more simple the apparatus the better. No engineer or mechanic would employ three wheels where two would suffice, and nothing can be so simple as the ordinary cable key, nor anything less liable to get out of order, and these are very strong recommendations in its favour, apart from its practical superiority.

JAS. GRAVES.

Valentia, December 9, 1879.

The PRESIDENT: I must say I thoroughly appreciate Mr. Varley's objection to entering into an elaborate discussion of the paper of Mr. Willoughby Smith, from having heard it read, and with no opportunity of seeing the paper. I think I may say this paper, the discussion on which will be carried on at the next meeting, will be available on application at the office before the next meeting, on the 26th of this month, and any gentleman who wishes to examine Mr. Willoughby Smith's paper will, I hope, have the opportunity of doing so. At our next meeting we hope to have a paper, which has been very much looked forward to, by Mr. Cowper, on the telegraphic pen, and I believe the best arrangement we can find is this, to carry on the discussion of Mr. Willoughby Smith's paper this evening as far as we can. Perhaps some gentleman would like to make remarks without the advantage of seeing *the paper in print,* and the discussion might be continued on the

26th, till 9 o'clock, when it will be necessary to close it, in order that Mr. Cowper's paper may be read. At the same time, I am sure every one here would be glad to hear Mr. Varley's observations, under the conditions he has mentioned, which, I feel certain, you will readily comply with, that he should be allowed to supplement them on a future occasion.

Mr. VARLEY (having taken his station at the black board), said: I will begin with the second paper first, because that will clear up some of the points I shall have to allude to by and by.

Mr. Graves in speaking about the advantage of "curbing," and its disadvantages, says, where a motion to the left is used for a *dot* and a motion to the right for a *dash,* with the curb key you get a plus current followed by a minus current for the dot, then another minus followed by a plus for the dash (this definition of curbing is not correct; it is explained further on). This is exactly what a condenser at the sending end of the cable produces.

It is not easy at a glance to see how it comes about, yet when calculated out, it will be found true, that when one condenser only is used upon a cable, it matters little whether the condenser be at the sending end or at the receiving end, the result, so far as the signal is concerned, being practically the same.

For argument's sake let us assume that the condenser is at the sending station, between the key and the cable, and further assume that the condenser is very small in capacity compared with that of the cable. In practice it always is considerably smaller than the cable.

On depressing the key the current from the battery will charge the condenser nearly to its full extent, after which no current flows into the cable. During the charging of the condenser, a positive current of short duration has flowed into the cable to produce the signal; the moment the key is raised the condenser will discharge itself, and having been charged to the full power of the battery, the result is a negative current of the same power as the positive current first sent into the cable. Therefore, when a dot is made, a positive current is sent into the cable, and when the key is raised a negative current is sent, in other words a complete

reversal, so that in point of fact the condenser has effected the so-called curbing, which Mr. Graves has been attempting by means of a reversing key.

A good deal of misapprehension with regard to curbing has arisen from a confusion of terms. The simple reversal of the current is not usually termed a curbed signal. True curbing consists of sending a succession of currents, from 3 to 5 or more, of such duration that they shall produce at the distant end of the line one small wave, which can be rapidly followed by others, because these reversals discharge the sending end of the cable before the signal has arrived at the receiving station.

Sir William Thomson's arrival curve shows that if the battery, when connected through the cable for a long time, will produce a maximum current of 100, then after a period of time, which he calls a, and which period varies with the resistance of the conductor, and the electro-static capacity of the insulator, no sensible current will have reached the receiving station, see Table No. 2.

TABLE No. 1.

Strength of Current at the Receiving Station after contact with the Battery for a period of

a	*a* 0·01	*a* 0·1	Difference.
	and then Cable put to earth.		
	E = 10 Volts.	E = 1 Volt.	
1·0	0·16 ÷ 10,000	0·11	0·05
1·2	0·6	0·48	0·12
1·4	1·5	1·26	0·24
1·6	2·8	2·45	0·35
1·8	4·3	3·9	0·4
2·0	5·9	5·6	0·3
2·2	7·5	7·2	0·3
2·4	9·0	8·7	0·3
2·6	10·3	10·0	0·3
2·8	11·4	11·2	0·2
3·0	12·3	12·1	0·2
3·2	12·9	12·8	0·1
3·4	13·4	13·3	0·1
3·6	13·7	13·6	0·1
3·8	13·8	13·8	0·0
4·0	13·8	13·8*	0·0
4·2	13·7		0·0
4·4	13·6		0·0
4·6	13·4		0·0
4·8	13·1		0·0
5·4	12·0		0·0
5·9	11·0		0·0
7·0	9·0		0·0
8·1	7·0		0·0
9·6	5·0		0·0
12·8	3·0		0·0
13·4	2·0		0·0
16·5	1·0		0·0
19·4	0·5		0·0
23·5	0·2		0·0
26·6	0·1		0·0

* *Maximum at 3·84 a.*

Curve No. 1, diagram No. 1, shows the rate of arrival and the amplitude of the signal wave at the distant end of the cable.

The horizontal line shows the periods of time after the first moment of contact with the battery, the vertical lines the strength of the wave. The maximum current produced by a long contact with the battery being 1,000 inches on the diagram.

The period a varies with each cable.

Curve No. 1, diagram 1, shows the effect produced by first connecting the cable to a battery for a very short time—0·01 a—and immediately afterwards connecting the cable to earth.

The strength of the signal is 0·001384, and this strength is reached after a lapse of time of 3·84 a.

Curve No. 2 shows the wave produced by a *curb* of four alternate currents. The wave, it will be seen, rises more rapidly and stops more abruptly than wave or signal No. 1. This wave No. 2 produces, by means of a condenser at the receiving end, a wave of the form shown by curve No. 3.

The curb producing signals Nos. 2 and 3 consists of four currents of the following durations:—

1st	current	positive	during	0·203 a.
2nd	,,	negative	,,	0·247 a.
3rd	,,	positive	,,	0·100 a.
4th	,,	negative	,,	0·050 a.
and cable immediately put to earth			,,	0·400 a.

Curve No. 4, diagram No. 2, is taken from col. 3, Table No. 1, viz: cable to battery for a period of 0·1 a, and then cable to earth.

The curves No. 5 show the effect of repeating the signal at intervals of a duration.

The first or lower curve is that which would be produced by one contact with the battery, *i.e.*, the letter E.

The next above it is that produced by two contacts, *i.e.*, the letter I.

The uppermost curve is that produced by three contacts, *i.e.*, the letter S.

The result is one large wave, and not three small ones: they *have run the one* into the other, and have lost their identity.

The following table shows the strength of current at the receiving end for different periods of time, supposing the battery to be connected between the earth and the cable at the sending end.

TABLE No. 2.

Thomson's arrival curve for periods of a.

$1\ a$	$= 0{\cdot}016$	$8\ a$	$= 68{\cdot}43$
$2\ a$	$= 2{\cdot}46$	$9\ a$	$= 74{\cdot}87$
$3\ a$	$= 11{\cdot}99$	$10\ a$	$= 80{\cdot}02$
$4\ a$	$= 25{\cdot}35$	$13\ a$	$= 90{\cdot}00$
$5\ a$	$= 38{\cdot}75$	$16\ a$	$= 95{\cdot}00$
$6\ a$	$= 50{\cdot}56$	$20\ a$	$= 98{\cdot}00$
$7\ a$	$= 60{\cdot}41$	$\infty\ a$	$= 100{\cdot}00$

If a current of $0{\cdot}01\ a$ duration be sent into the cable, and the cable immediately put to earth, a minute wave at the distant end will be produced, which rises to a maximum in $3{\cdot}84\ a$, and will take about $12\ a$ to subside sufficiently for a second signal to be sent (*vide* table No. 1 and diagram No. 1).

This table No. 1 shows the effect of a very short current of $0{\cdot}01\ a$, as compared with one ten times as long, or $0{\cdot}1\ a$.

With the former $(0{\cdot}01 a)$ a potential of 10 is calculated out, with the latter $(0{\cdot}1\ a)$ a potential of $1{\cdot}0$ is calculated out.

The maximum strength of the signal is $13{\cdot}83 \div 10{,}000$.

The signal begins to be visible only after the lapse of about $1{\cdot}4\ a$ or $1{\cdot}3\ a$.

It practically rises near enough to its maximum at $3{\cdot}4\ a$ or $3{\cdot}3\ a$.

This table shows also, that with currents of such short duration the speed cannot be increased by diminishing the duration of contact and increasing the power, the greatest difference being at $1{\cdot}8\ a$.

They both reach their maximum practically at the same time, having a difference of only $0{\cdot}04\ a$ at the commencement of visibility.

The former at $1{\cdot}26\ a = 0{\cdot}81 \div 10{,}000$
The latter at $1{\cdot}30\ a = 0{\cdot}82$

The cable once charged has to discharge itself through its two ends to the earth.

If a curbed signal of 4 currents be sent into the cable, and their duration be as follows:—

1st current	...	...	0·213 a +
2nd ,,	...	...	0·247 a −
3rd ,,	...	...	0·092 a +
4th ,,	...	...	0·048 a −
and then to earth ...		...	0·400 a

it will be found that the charge in the cable at the termination of the signal will be approximately as indicated in diagram No. 3. This diagram is purposely exaggerated in order that the smaller waves may be visible in this room.

Table No. 2 and diagram No. 1 shows that the signal does not begin to appear till 1·3 or 1·4 a has elapsed, but the reversals have been completed in 0·6 a, that is, 0·7 a before the signal begins to appear practically at the receiving station.

This combination of currents leaves the cable at 0·6 a approximately, as shown in the distorted diagram No. 3. The first section negative, the second positive, but of lower potential, the third negative, of still lower potential, and the fourth positive, of much lower potential, the remainder of the cable being neutral.

The speed of charging and discharging a cable varies inversely as the square of its length.

By means of the curb signal the cable has been electrically divided into short positive and negative sections.

The various waves of positive and negative electricity combine together and discharge the cable so rapidly at the sending end, that after a period of 0·4 a has expired, a new signal can be commenced, 0·3 a, before the signal begins to be visible at the distant end.

If an attempt be made to send signals without this interval of rest of 0·4 a (in all a period of 1·0 a), the waves will run together and produce confusion.

The foregoing indicates a definite limit, beyond which nothing more can be done to increase the speed.

By inserting a condenser at each end of the cable, *curb* signals *may be made to follow* each other distinctly at an interval of 1·3 a,

or even 1·1 *a*, with sufficient distinctness to be read by expert hands, and this is the limit of speed obtainable by any means through a perfect cable. (*See diagram No.* 1, *curves* 2 *and* 3.)

It is this cutting the cable into sections, with positive and negative charges, that constitutes the true curb signal.

With condensers inserted at each end, and the ordinary key, this speed is approached, but not nearly reached, and then only at a sacrifice of distinctness. The eyes, however, of very expert telegraphists can read very indistinct waves, and in this way the speed 1·7 *a* per signal can be obtained without the curb key.

It might be interesting to this Society to know that I worked a telegraph on the South Devon Railway from October, 1847, till October, 1848, with what Mr. Graves terms a curb signal—that is, a positive current, followed by a negative current, for each signal, and this was found exceedingly useful in removing the permanent magnetism of the receiving electro-magnet.

In 1855 (*vide* patent 1318), and in 1856 (*vide* patent 3059), I patented a system of overcoming the effects of electro-static induction in submarine cables by using a positive current of short and long duration, for producing dots and dashes, these currents being each followed by a negative current to discharge the line.

It was by this means that I opened direct communication between London and Berlin in 1855.

By a reference to the patents in question (which are too lengthy to be produced now), a full description will be seen of the effects produced by electric currents on long cables, although at the date of the patents there were no cables in existence of greater length than 120 nautical miles.

In 1858, Sir William Thomson patented a triple current system of curbing. He used three currents of equal duration, but of different potentials.

By experimenting on the artificial line, I found that 4 or 5 alternate currents of equal potential and varying durations were much better than the three above mentioned; also that Thomson's 3 currents were a great improvement upon the mere reversal.

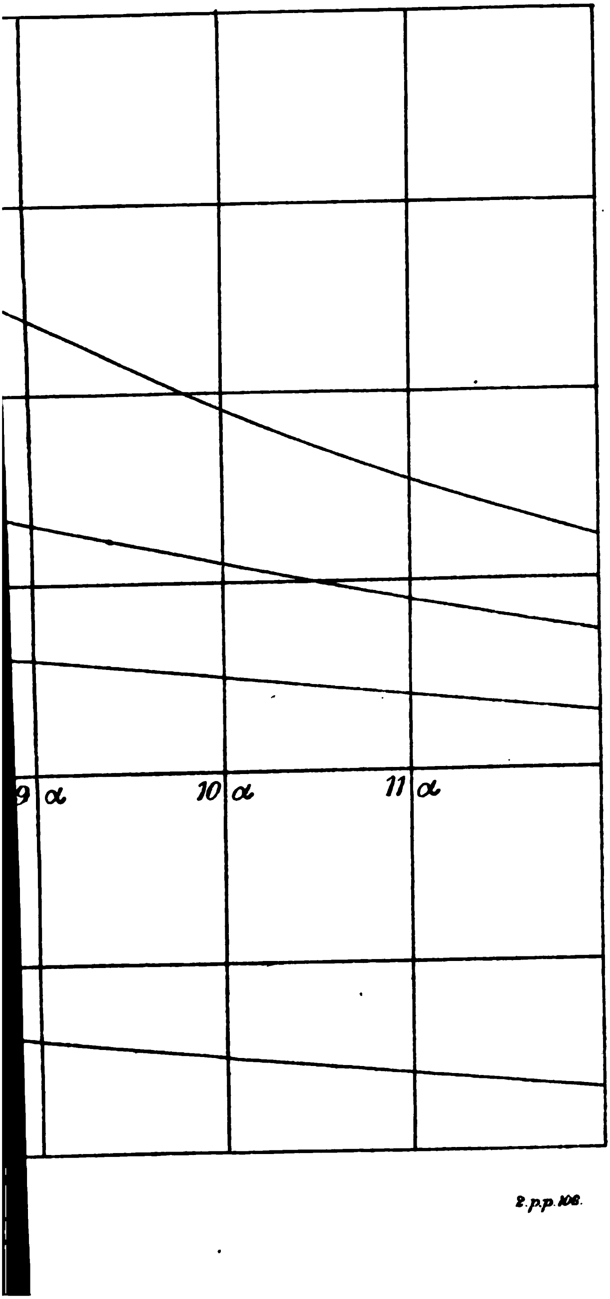
The alphabet in use upon the Atlantic and other long deep sea cables *is the single needle alphabet* of the late Electric and

International Telegraph Company, and was introduced about the years 1856 and 1857, and was adapted from tinental alphabet, which is quite different from the Am Morse alphabet.

Mr. Willoughby Smith in his paper has given me a rap knuckles with regard to the specification of my patent of 1

In that specification it is stated that no benefit woul rived from the condenser at the receiving end if the ance coil or shunt were not applied. This is an error. The Agent recommended me to apply to Sir William Grove, the to settle the specification, and he was a considerable time and the date of filing the specification was near at hand. result was an incorrect statement, which was not discove time by me. It should have been that not so much be derived from the condenser alone as when the shunt is emp The reason is as follows:—

If the condenser alone be employed—and for argument's sa will suppose the galvanometer to have a very small or no r ance—on the current arriving at the distant end it will charg condenser to the same potential as itself, when no further cu will pass from it through the galvanometer to earth. If a s be employed of such magnitude that it will carry off one ha this potential, it will be seen that on the arrival of the first por of the wave, when the condenser is uncharged, there will be li or no resistance between the cable and the earth, because the c denser offers scarcely any. Therefore, the first part of the w will produce nearly as strong a signal through the galvanome and condenser as if there were no shunt; but as the condenser b comes charged a current of increasing power flows through t shunt, and the condenser will cease to charge some time before t potential has arrived at the previously stated maximum. The co sequence is that the first part of the wave only is observed, and t after part cut off. The speed is thereby considerably increase If a succession of small positive currents be sent into the line as i rapid working, the result at the distant end will be a long positiv wave with an undulating surface or curve. In fact, the signal will be like small ripples upon a large wave. The effect of a



2. p.p. 108.

condenser is to separate the rapid little ripples from the large wave, and as the spot of light is thereby limited in its motion by cutting off the large wave, a more sensitive galvanometer can be used, and thus the ripples become magnified into clear distinct signals. One of the very great advantages of the condenser is that the disturbances arising from earth currents, and which with a sensitive galvanometer would throw the image off the scale, are almost entirely removed. Earth currents seldom change from positive to negative in less than four or five minutes. The charging of the condenser by a current that changes potential slowly is slow, and therefore although the earth current may be many times as strong as the signal battery, yet the comparatively rapid changes of potential produced by the short signals charge and discharge the condenser much more rapidly, and thus the signals arising from the short rapid changes of potential of the signalling current, are separated from the greater but slower potential changing earth current wave. By this and other means described in my patent the signals are dependent upon the *increment* and *decrement* of the current, and not the current itself.

Suppose the earth current to have a potential of 100 volts, that is, suppose it to go from nothing to 100 volts in five minutes, or 300 seconds, and the signal to be produced in a quarter of a second from a battery of 10 volts, the rate of charging the condenser in the latter case will be 120 times more rapid than the charge due to the earth current.

The large slow wave produced by a succession of positive currents is like the slow changing earth currents—the condenser cuts off nearly the whole of the large slow wave, and separates distinctly the small rapidly varying ripples or signals. The gain of speed from this source is very considerable.

During magnetic storms, I have seen the potential of an earth current between Ipswich and London rise as high as 120 volts, and on some shorter lines even greater differences of potential in proportion to their length. It is fortunate that the differences of potential in submarine cables are generally very much weaker during magnetic storms than upon land lines.

The use of a condenser at both ends, instead of at only one end of the line, increases the speed about 33 per cent. The cable between Brest and St. Pierre has a capacity of a little over 1,000 microfarads. The cable and galvanometer used had a resistance of about 8,000 ohms. On this cable the above was proved.

If a condenser of 100 microfarads be inserted between the battery and the key, and that battery have a potential of 10 volts, the charge thrown into the cable of 1,000 microfarads through the condenser when the key is held down will raise the potential throughout the cable to 1 volt, and therefore, to charge the cable to the same extent as the battery without the condenser would have done, it would be necessary to employ 100 volts of battery to produce a charge of 10 volts in the cable.

At the first moment of applying the battery the current rushing from the condenser into the cable has the same power as if no condenser were inserted. This current rapidly dies away towards nothing as the condenser becomes charged, and as it is only with the first impulse that the signal at the distant end is made, the decrease of action on the galvanometer at the distant end is not so much as would at first sight appear, and consequently it is not necessary to increase the battery tenfold to get the same strength of signal—30 volts being amply sufficient. The condenser, therefore, prevents the cable from being submitted to a potential so high as would have been the case with the 10 volts without the condenser, and yet by cutting off the latter portion of the current causes a greater rapidity in the completion of the little wave or signal at the distant end.

If, for argument's sake, a condenser of 10 microfarads were used, a battery of 1,000 volts would be required to produce in the cable a charge of 10 volts, and if this battery have little or no resistance the condenser will receive its charge very rapidly indeed. When the key is raised and the cable is connected to the earth, the condenser, which is now charged to 1,000 volts, discharges itself, and in so doing discharges the cable as quickly, or strictly speaking, quicker than the cable had been charged, and these sharp measured positive currents are sent into the cable followed by equally

sharp measured negative currents, hence the gain of speed and distinctness of signalling.

Another advantage which arises from the use of condensers at each end of the cable, is that by attaching the cable through a large resistance to the negative pole of a battery, the positive pole of which is connected to the earth, the cable is always kept negative to the ocean. This system is employed with the French Atlantic Cable, in which there was a small defect, arising from a puncture through the gutta percha. The cable is kept at a potential of about 40 volts negative to the ocean, but as the insulation of the condensers is very great no perceptible current is produced by this battery upon the receiving instruments at the stations.

In practice, however, when the sending key is depressed, a charge equal to say 2 volts is added to or taken from this negative charge of 40 volts. If the current sent into the condenser be a positive one, it will reduce the potential in the cable from 40 to 38, a difference of 2 volts, and the signal wave at the receiving end has the same strength as that which would have been produced if the cable had been neutral to the earth, and 2 volts positive used for the signal. When the other key is depressed, the charge is 2 volts negative, and the negative charge in the cable is increased from 40 to 42 volts, producing a difference of 2 volts in the opposite direction. Thus, although the cable is kept powerfully negative to the ocean, the positive and negative signals from the condenser are precisely the same as if it had been neutral. The object in keeping the cable negative to the ocean is to prevent the decomposition of the conductor, which invariably takes place when a positive current flows from the copper into the sea-water, the result being the formation of chloride of copper, and this decomposition in the space of a short time eats away the conductor, and then no further signalling can be carried on. When some years afterwards the fault in the French cable was cut out, the conductor, where exposed to the water, was found perfect and bright, the protecting battery having completely prevented all destruction.

Mr. Willoughby Smith states that in 1860 I reported to the

Atlantic Telegraph Company that a conductor, weighing 1,150 lbs. to the knot, and covered with gutta percha to the extent of 1,020 lbs. per knot, would be required to get 8 or 9 words a minute. I can find no record of having made such a statement.

In 1854, in a paper which I read before the British Association, I estimated something like these dimensions would be required. At that time the conductivity of commercial copper was nothing like so good as that which is now used, many parts of the first Atlantic Cable laid in 1858 having a conductivity varying between ¼th and ⅓rd only of that of pure copper. Mr. Smith however does not state, and perhaps is not aware, that in 1863 I was called upon to determine the dimensions to be adopted for the cable. I had, prior to this, constructed my artificial line, and Mr. John Chatterton, at my request, prepared samples of insulated conductors, the copper weighing 300 lbs., and the gutta percha 400 lbs. per knot, the copper being of high conductivity. We are indebted to Sir William Thomson and the late Dr. Matthiessen, the first, for discovering how very imperfect a conductor commercial copper is, the latter for the means of procuring pure, or nearly pure, copper wire.

In 1854 the reflecting galvanometer was not invented. The nearest approach to it was my galvanometer relay. The longest submarine cable then laid was from the coast of Suffolk to Holland, less than 120 knots.

In 1858 I was sent to Valentia to test the Atlantic Cable, which had ceased to act, and there I saw slips of paper recording signals sent through that cable by means of Mr. Whitehouse's apparatus. I could find no slips giving distinct words at a greater rate than about 1·1 words a minute. I was shown some slips when the clerks were conversing together, using abbreviations, and very indistinct signals, in which a speed of 2½ words were obtained.

The only long subterranean line at my command was the underground line laid by the Electric Telegraph Company between London and Liverpool. The insulation of these wires was very imperfect, as was the conductivity of the copper, and

before I had constructed my artificial line, we had no data upon which to make any proper estimate.

Before 1862, I had ascertained by the artificial line that 8 *distinct* words a minute could be obtained through 300 lbs. of copper and 400 lbs. of gutta percha to the nautical mile between Ireland and America, with a cable 1,900 nauts. in length, without condensers.

This statement was published by the Atlantic Telegraph Company on the 26th March, 1862, when in reply to the Rt. Hon. Mr. Stuart Wortley, I said I expected to obtain ultimately 16 words per minute. I was then working upon my condenser system.

Sir W. Thomson, who had been experimenting upon my artificial line, joined me in pledging our words to transmit 12 words a minute through the cable I had proposed of 300 lbs. copper and 400 lbs. gutta percha to the knot.

I was not then prepared to expect that telegraphists would become so expert at deciphering indistinct signals as to be able to increase the speed to 20 words, or even more. I am informed that in practice they attain a working speed of about 14 words a minute between Ireland and Newfoundland.

When the cables belonging to the Anglo-American Telegraph Company were both broken, and the only means of communication with America was the one cable of the French Atlantic Telegraph Company, I spent some time at Brest, where, by reducing the resistance of the galvanometer, and also that of the sending batteries, which were ridiculously great, a speed was maintained for a long time of 17 words a minute, notwithstanding that the cable is very much slower on account of its length than the cable between Ireland and Newfoundland. The signals, however, were too indistinct for my less practised eyes to decipher, but the experienced hands at each end were able to decipher them with remarkable accuracy.

Some months before the expedition started in 1865, the clerks who were to work the cable were educated by me at my house upon my artificial line.

Before the expedition sailed in 1865, the Atlantic Telegraph Company invited *competitors to test* their apparatus upon the

cable coiled on board the *Great Eastern*. A long cable, when coiled in large masses, as was the case in the three tanks on board the *Great Eastern*, is much slower in speed than when laid out, in consequence of the magneto-electric induction between the upper and lower coils of the cable in each tank. In this competition Mr. C. V. Walker, Telegraph Engineer to the South Eastern Railway Company, and past President of this Society, was appointed umpire. The only competitor besides us was Mr. C. V. de Sauty, Electrician in Chief of the Telegraphic Construction and Maintenance Company, and his staff, with the usual key at one end, and a Thomson's reflecting galvanometer at the other.

As Sir William Thomson and I were in joint partnership, we protested against the unfairness of the competition, seeing that the Telegraph Construction Company were competing with us, using our inventions, the reflecting galvanometer, &c.

The result of the trial was as follows :—Without the curb key and condenser, from 2¼ to 2¾ words a minute were obtained, but the errors made were so numerous that the messages were frequently unintelligible.

With the curb key and condenser a speed varying from 5¼ to 5¾ words a minute were obtained, and the errors were few (*vide* Mr. C. V. Walker's report to the Atlantic Telegraph Company in 1865).

In 1866, Sir William Thomson and I were, at the request of the Telegraph Construction and Maintenance Company, and with the consent of the Atlantic Telegraph Company, appointed Consulting Electricians to the Telegraph Construction and Maintenance Company, and we had to settle the methods to be adopted for testing the 1866 cable. Mr. Smith's gelatine condenser or big resistance (for it was in fact a condenser of small capacity and imperfectly insulated) was adopted to enable us at Valentia to measure the strength of the current, and signal it back to the *Great Eastern* at stated intervals. All attempts to pass words by means of this gelatine resistance were too slow to be practically available, and my condenser system at Valentia, with a battery and key was adopted, as shown in figure 10 of Mr. Smith's paper. The condensers, keys, and battery used were made by myself, and were joined up by

myself at Valentia. It was my system. This got over the great retardation due to the resistance of the gelatine condenser, and a speed of five or six words a minute was obtained through the long coiled cable. When the cable was laid no change was made at Valentia, and the apparatus used in Newfoundland was transferred from the *Great Eastern* to the office at Heart's Content.

The reflecting galvanometer used at Valentia was prepared by Sir William Thomson and myself, and was our property. The condensers at each terminus were made by myself, and were my property, and were connected up, as indicated, by myself.

The method adopted was mine and Thomson's in every respect, and not that of Mr. W. Smith.

A large number of messages were sent from Newfoundland to Her Majesty's Ministers and other important personages, announcing the successful completion of the cable. I read off the great bulk of these messages, being at that time probably the most expert hand at reading, having had two or three years' experience. The alphabet I selected, and which has ever since been used upon all long cables, was the single needle alphabet of the Electric Telegraph Company.

The first person who proposed to work duplex through a long cable was probably Sir William Thomson (see his patent of 1858).

Throughout the paper Mr. Smith calls the sawdust battery, now so commonly used, the Menotti battery. Menotti used sand and not sawdust.

The first person who used sand was Sir William Fothergill Cook, who employed it in 1846 on the South Eastern Railway, in the acid batteries.

In 1853, Fuller used sand in the Daniell's battery.

In 1855, I patented the "Gravity Battery" and "Sulphate of Mercury Battery," the negative plate being placed at the bottom of the cell, the positive or zinc plate being placed over it.

In my patent of that year, No. 2555, a description will be seen.

These batteries were largely used by the Electric Telegraph Company, and I employed clay to assist gravity in keeping the two solutions apart.

In 1860, Sir William Thomson patented the use of sawdust over the horizontal copper plate, this sawdust separating the zinc plate from the sulphate of copper and the lower or copper plate.

The so-called Menotti battery is simply my gravity battery, as improved by Sir William Thomson in 1860 (see his patent).

Mr. Smith in his paper states that as the curb key and other arrangements of mine have not come into general use, he presumes we found no advantage from them. We demonstrated in 1865 on board the *Great Eastern* the great advantage of curbed signals, that is, employing four or five currents to produce one sharp signal at the distant end. The real reason why they did not come into use was because the Anglo-American Telegraph Company would not allow the curb key and artificial line, which I had sent out by their instructions to Newfoundland and Valentia to be joined up. In fact they excluded Thomson and myself, and our more recent apparatus from their Telegraph Stations, turning the door on their two best friends, and when an impartial history is written, when we no longer exist, it will be known that, but for our inventions the Atlantic Cable would not have been able to transmit a remunerative traffic at the present rates. A cable that will give half a word per minute with the Morse instrument, will give 13 words when the mirror galvanometer and condensers at each end are used.

The mirror galvanometer, enabling signals to be read with a shifting zero, and without any fixed material stops to limit the play of the galvanometer, the condenser cutting off the earth currents, and greatly expediting the signals, have made long deep sea cables commercially successful.

Mr. W. Smith's fig. 2, representing my method of working, patented in 1860, is incorrect. The upper or middle contact should be connected with the earth.

At page 20, Mr. Smith says, "I am not aware that any one of these systems of Mr. Varley's was practically applied to a submarine line; but if so, I imagine the results were not satisfactory, for on the 6th July Messrs. Thomson and Varley conjointly patented Improvements in Electric Telegraphs," viz., the curb key.

I would like to ask how I could test my methods upon a long

submarine cable when there was no submarine circuit in existence of more than a few hundred miles in length.

The reflecting galvanometer and condenser are used on all long cables, and are the inventions of Sir William Thomson and myself. Figs 9 and 10 are incorrect; they do not show the "potential divider" of Thomson, nor the "potential subdivider" of C. F. Varley, nor are the switches correctly drawn.

Mr. Smith's last paragraph, page 34, is just the reverse of what is the fact, as I have already explained.

In page 41, Mr. Smith says: "When, early in 1752, Mr. Kinnersley placed the needle of a compass on a long pin, and holding it in the atmosphere of the prime conductor at the distance of about three inches, found it to whirl round like the flyers of a jack with great rapidity, he had no conception that he had within his grasp a discovery which sixty years afterwards immortalised Professor Oersted."

This attraction was electro-static, not electro-magnetic.

No galvanometers existed till Schweiger in 1820 invented the galvanometer.

It requires a very sensitive galvanometer to show the current from even a very large frictional machine. In Mr. Kinnersley's experiment a brass or wooden needle would have answered as well as a compass needle. This movement had nothing whatever in common with Oersted's discovery.

With reference to keeping up a continuous test for insulation through a cable, the first person to realise this was Sir William Thomson.

Thomson, W. Smith, De Sauty, and I discussed this subject freely in 1865 on board the *Great Eastern* when returning from the unsuccessful trip.

Thomson hit upon the plan for keeping up the insulation test and signalling to the shore by means of his Quadrant Electrometer. I then pointed out how a condenser would increase the speed of working.

On arriving in England, Sir W. Thomson sent in his proposition to the Atlantic Telegraph Company, who published it. Mr. Willoughby Smith took out a patent, in which he proposed using

a condenser similar in all respects to my system of 1862, except that he used an imperfectly insulating one, while I used as perfect an insulator as I could get. In his patent he calls it a condenser.

Neither Sir William nor I attempted to upset his patent, although we could each of us have done so.

The PRESIDENT: We are obliged to Mr. Varley for what he has told us; and I wish to say, as the hour is rather late, we will now adjourn the meeting, and perhaps it may be convenient to continue the discussion for half an hour or so on the 26th inst.

The meeting then adjourned.

The Seventy-fourth Ordinary General Meeting was held on Wednesday, February 26th, 1879, at the Institution of Civil Engineers, Great George Street, Westminster, Lieut.-Colonel Bateman-Champain, President, in the Chair.

In the absence of the Secretary, the preliminary business was conducted by Professor Ayrton.

The President: We will now commence with the adjourned discussion of Mr. Willoughby Smith's paper, read at the last meeting.

CONTINUATION OF THE DISCUSSION ON MR. W. SMITH'S PAPER ON THE WORKING OF LONG SUBMARINE CABLES.

Mr. C. F. Varley here continued his remarks, but for facility of reference they have been printed together with those made by the same gentleman at the previous meeting.

In connection with the discussion on Mr. Willoughby Smith's paper, the Secretary announced that Mr. Graves, of Valentia, sent the following observations:—

Mr. Willoughby Smith's paper recalls to my mind many of the circumstances connected with the early working of the Atlantic Cables, but as Mr. Smith has had very little personal experience in the commercial working of those cables, and none whatever in the actual working of the siphon recorder, perhaps I, having been appointed Superintendent of the Valentia station in 1865, before either of the cables were laid, and having held that position ever since, may be allowed to make a few remarks upon the subject with a certain amount of authority, being the result of my own practical experience during the last 13½ years.

In connection with our early days of working from our permanent station, I would call attention to the discussion upon my paper "On Vibrations due to Earthplates," in which Mr. Bordeaux endeavoured, perhaps unintentionally, to transfer the credit of the result of my long series of experiments from me to Mr. Varley.

On 4th January, 1876, I sent a letter, calling attention to this, to the Secretary, requesting its publication, but it was never published in the Society's Journal. In that letter I remarked that it was "only reasonable to suppose that the vibrations due to earthplates on the French Atlantic Cable were not observed upon the mirror instruments until *after the cable was laid*, and then the same remedy (referred to by Mr. Bordeaux) was applied as had been discovered and applied by me to the cables working from Valentia (the 1865 and 1866 cables) *one year previously*, as detailed with dates in my paper."

Another point I would mention is, the method by which the most suitable length of suspension for the mirror receiving galvanometers was arrived at, soon after the successful completion of the 1866 cable. Mr. T. E. Weatherall and I made, conjointly, a series of experiments with various lengths of suspension, commencing from about 2 inches above and below the mirror, which was suspended in a frame fixed in a slot between the coils of the galvanometer. The full length of the suspension fibre was too delicate and too wandering for rapid signalling with the controlling magnet outside the case at a long distance from the mirror-magnet. We fixed in the frame small blocks of gutta percha, pressing against the fibre above and below the mirror, and by numerous trials gradually brought the blocks nearer and nearer to the centre, until we had reduced the length of the fibre in action to about the sixteenth of an inch above and below the mirror, when the signals were at their best, but still the controlling magnet was too far off.

We then dismounted the coil of a marine iron-clad galvanometer, and placed it in a gutta percha battery-jar, with a hole cut in front for the passage of the light to and from the mirror, and two small rectangular holes at back for the two poles of a flat horse-shoe magnet to pass through. We found that by approaching the magnet as nearly as possible to the mirror, the definition of the signals was much improved, and they became sharply defined with an almost dead-beat movement; and, if I remember rightly, it was this arrangement which gave us in those early days the speed, referred to by Mr. W. Smith, of 25 words, or 125 letters

per minute. When the best positions were found for the gutta percha blocks they were taken out, and brass blocks were soldered into the frame, where they remain to this day.

The above result led me to design a special form of mirror tube, and for several years past I have had in use at this station two galvanometers with mirror tubes made to my drawings, copies of which I send herewith.

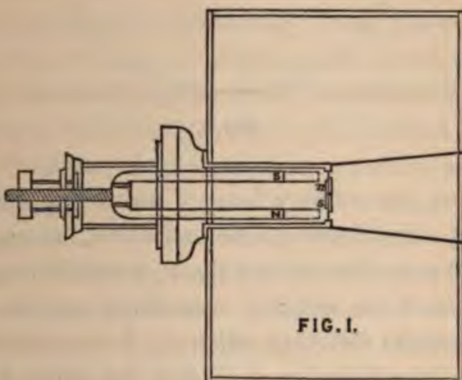


FIG. I.

Fig. 1 shows the horizontal section with an enclosed permanent magnet in the tube, adjustable for about three-tenths of an inch, with its N. and S. poles close to and opposite the S. and N. poles of the mirror-magnet.

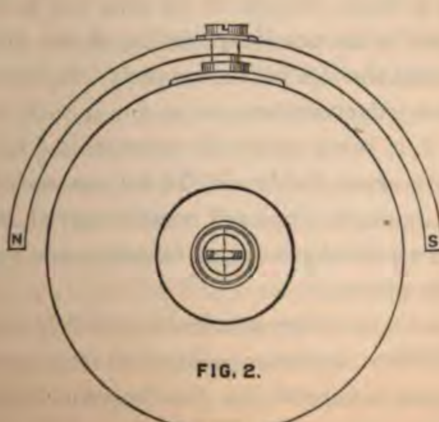


FIG. 2.

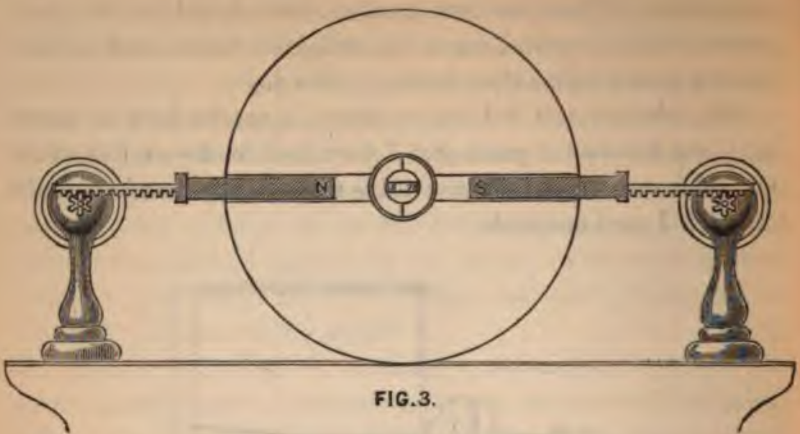


FIG. 3.

Fig. 2 shows the ordinary form of controlling magnet outside the brass case. Fig. 3 shows a method which, though I have never used it, should prove better than fig. 1, if carried out. But in fig. 1 the retention of the ordinary controlling magnet, as in fig. 2, overcomes the slight difficulty which fig. 3 is intended to remedy without it, viz., the difficulty of getting the poles of the enclosed permanent magnet to attract *equally* the two poles of the mirror-magnet, in order to keep the mirror strictly parallel. The controlling magnet in fig. 2 does this effectually, and maintains a proper zero position, although it is too far off for good definition by itself, this being left to the enclosed magnet. The double bar magnets in fig. 3 would provide for all this, but it would be more difficult to adopt in the practical winding of the coils unless made in two halves, and the slot for the magnets left between them.

One of these galvanometers, as in fig. 1, with the controlling magnet in fig. 2, is being used at the present time for "duplexing" on the Anglo-American Cable of 1874 for the mirror system, and the first paid messages duplexed were received upon it. This form of tube is a perfect success, as several years' practical experience sufficiently proves.

Not long since, in a correspondence published in the *Times* and elsewhere, Sir James Anderson referred to certain speeds of work attained and attainable on the Eastern cables in term of words per minute," and Mr. W. Smith has fallen into the same

vague mode of expression, in referring to Mr. Varley's "six words a minute" for the curb key on board the *Great Eastern*, and "thirteen words a minute" for his gelatine-condenser system before he left Heart's Content, and later on, "25 words a minute received at Valentia with perfect ease." It is only when he gives the number of contacts per minute for the latter speed (*i.e.*, 375) that the general reader finds out that the term "word" means 5 letters of 3 signals each.

Nothing can be more misleading to the general public than giving the speed of a cable in the vague terms of "words per minute," and it would be well for all such data in future to be given in "letters per minute," which would have a definite meaning, whereas "words per minute" is as indefinite as anything can well be.

I remember it used to be a standing joke some years since that the following words were used as a test for "speed in words per "minute," viz., "It is so hot, so hot it is, it is so very hot," and a fabulous speed was of course obtained, and no one could gainsay it, for of such words it would be an easy matter to obtain a speed of 50 words a minute through either of the Anglo-American cables; but when we come to such words as the following, taken from actual messages paid for at three shillings per word, the same prices as an indefinite article or a personal pronoun, it will not be expected that anything like a speed of 50 words a minute could possibly be transmitted.

Specimen of commercial message "words:"—Agedicavissent, streonshalh, schiagraphy, apostillerabunt, yruracbat, decaimiento, entrementimientos, manciparemus, frommigkeit, discalcantibus, abbronzavamo, obnubilitate.

I think this conclusively shows the necessity of abolishing once for all the "words per minute" basis, and adopting the "letters per minute" as a universal standard for all speed comparisons. I notice further on that Mr. Smith records "19 words a minute on the Malta and Alexandria Cable of 1,330 miles, or thereabouts." That, I take it, means 95 letters per minute. The Atlantic Cable can, with plain English words, add 40 per cent. to that speed, although over 500 miles longer, with the present system of working

with condensers at both the sending and the receiving end, for which purpose the switch S, in diagram 11 of Mr. W. Smith's paper is placed between C and G, instead of between K and C.

If *amplitude of signals only* were required, we should not use condensers at all, nor would it have been necessary to shorten the suspension fibres of our mirror galvanometers, but rather the contrary. Something more, however, has to be provided for. We do *not* want signals ranging over a scale of one or two feet in length, but we *do* require a short sharp signal, definite in its indications, and brought back as rapidly as possible to its zero position. Of course when a condenser is inserted at *one* end, more battery power is required for signalling, but the signals are improved, not in amplitude, but in definition; and if condensers are inserted at *both* ends, completely cutting off the cable from the earth, still more power is required for a given range of signal, but as amplitude is of secondary consideration (within certain limits) as compared with distinctness of beats, which is of primary importance, condensers improve, rather than mar, the signals.

A still further advantage of the condensers is, that while the cable is always insulated at both ends, the working is *never* disturbed by earth currents, and so effectually are these cut off that, although there may be a very high static charge in the cable, the operators are in blissful ignorance of the fact.

The landlines frequently show heavy "deflections," while the cables show nothing whatever. This is the great advantage derived from the use of condensers at both ends, and which led to their adoption, and although more battery power is required to work with than without them, yet our maximum limit of 20 cells is never exceeded. If with that power the signals are not readable, the receiving instruments are modified to obtain what is required.

It may be objected that we could work with less power if we had condensers inserted at one end only, and still reap advantage, as the station having the condenser in circuit (the receiving station) would be cut off from the effects of earth currents. I grant this, but my experience has shown that when the cables are highly charged with static electricity, on turning the switch after

sending or receiving, a strong dynamic current flows from the cable, and upsets the mirror or siphon, unless cut out by a short circuiting switch, and if a "call" be sent sharp, part of it is lost, and has to be repeated; and nothing of this sort is required with the condensers at both ends, while the extra battery power is a trifling matter, seeing that 20 cells are never exceeded under any circumstances.

There is another phase in the speed of cables which interests shareholders more than electricians. For comparative speeds the electrician would require to know the carrying capacity of the cable in *letters per minute*, but the shareholder's comparison is a very different thing. He, naturally enough, would ask "how many shillings per minute can you earn on the basis of your published tariff?"

And in order to ascertain this, it must be remembered that delays are introduced by the messages being written in other languages than English, that there is the unpaid preamble, that there are often code and cypher messages, and that when a message is composed of groups of figures, *every figure* by rule must be repeated back. Also the occasional hitches, misunderstandings, cases of simultaneous sending from both ends, with (on a single line) no means of stopping the one or the other that occur, as well as the large number of service messages for repetition, enquiries, and necessary official communications, must be borne in mind, as conditions that affect the commercial speed, although not the electrical speed, of a cable.

With regard to the question of "Mirror *versus* Recorder," I would remark that after four or five years' experience with Sir William Thomson's Siphon Recorder on the Atlantic cables, I have before me a message in which Mr. E. Weedon, the Superintendent at Heart's Content, estimates that two recorders are preferable to three mirrors, and although my own data do not warrant me in fully endorsing that opinion, I can without any hesitation say that three recorders are preferable to four mirrors, so that, taking Mr. Weedon's estimate, the economy of the recorder lies in saving one cable out of three, and taking my estimate, the saving of *one cable* out of four. It is well known

that the first cost and maintenance of the recorder are greater than those of the mirror; its construction is more complex, and requires skilled attention, but the certainty of its work, and the avoidance of repetitions from being able to look at a word more than once, and the reduced strain upon the eyesight, are all proved by the favour with which it is regarded by every member of the staff at both ends of the Anglo cables, which is an indisputable recommendation for it, and so long as a recorder is available, none of the staff would ever dream of going to mirror in preference. To work duplex on mirror would require three clerks at the instrument, but the recorder like the Morse would require but two.

I now pass on to diagram 19 in Mr. Smith's paper. The signals in fig. 1 show clearly both the charge and discharge. When the charge is followed by an opposite signal, or *reversal*, it is split up (*vide* letters *a*, *c*, *d*, *f*, *g*, *k*, *l*, &c.), but when the next signal is of the *same* polarity, it hollows out and sharpens the curves (*vide* latter part of *b* and *j*, and also letters *h*, *i*, *m*, *o*, *s*), and no clerk could read a message with such signals.

In fig. 2 the same phenomena are visible, but the amplitude is reduced by the addition of the cable, and the signals are unreadable except the letters of same polarity as *h*, *i*, *o*, *s*.

But as regards fig 3, which most concerns the subject of my paper read at the last meeting, if it proves anything at all, it proves the accuracy of my conclusions, although from the same data they are diametrically opposed to those of Mr. Smith, and I have only to request an examination of the alphabet there given, to prove that letters of the same polarity are improved by curb signalling, while all others containing reversals are split up and distorted, as witness for letters of same polarity, *e*, *h*, *i*, *m*, *o*, *s*, *t*, and parts of *b*, *j* and *v*, where the signals are formed by regular reversals; whereas on the contrary the letters *a*, *c*, *d*, *f*, *g*, *k*, *l*, *n*, *p*, *q*, *r*, *u*, *w*, *x*, *y*, *z*, would be all unreadable, if received in combination as words. No further proof is required to show the failure of the curb system as at present applied with one pair of currents for each signal.

Mr. C. F. Varley estimates *seven currents* as necessary for

discharging the Atlantic Cable. Sir Wm. Thomson says (Vol. V., p. 234, Society's Journal), that "a perfectly curbed signal could only be produced by using an *infinite series* of curbing currents alternate in direction, and each somewhat shorter than its predecessor. Of course such an arrangement is practically impossible."

Doubtless Sir William is perfectly right, and Mr. Varley approximately so, but curbing as hitherto actually tried on the Atlantic cables with a single pair of cams—one for marking, the other for curbing—is all wrong, as there is too little of that gradual tapering off of the charge so absolutely necessary for giving a succeeding signal of contrary polarity a fair chance of doing its duty properly.

It would in the present state of things be premature for me to make any reference to duplex, beyond saying that unless better signals than those shown in diagram 21 can be obtained on the Anglo cables, duplex could never be practically adopted. I have, however, already seen far superior signals to those in our present state of experience, both on mirror and recorder.

I have thrown these hurried remarks together since the receipt of the printed copy of Mr. Smith's paper, in order to be in time for the discussion, and want of time to write them more carefully must be my excuse, if they are not so connected and intelligible as they might otherwise have been.

JAS. GRAVES.

London, 27*th February*, 1879.

The Secretary of the
Society of Telegraph Engineers.

Dear Sir,

In reference to Mr. Willoughby Smith's paper on the Working of Long Submarine Cables, in which he states that the insertion of a resistance at the condenser tends to *decrease* the speed of signalling, I beg to remark, that resistance coils connected to a condenser in the manner described, were introduced some years ago by myself and Mr. Venndt, on one of the Great Northern Telegraph Company's cables, with the result of not only improving

the signals, but of *increasing* the speed of working by about 10 per cent.

Yours faithfully,

CHR. DRESSING, Associate.

The further discussion of this paper was postponed till the next meeting.

Professor AYRTON followed with a paper on "A New Determination of the Ratio of the Electro-Magnetic to the Electro-Static Unit of Electric Quantity."

A NEW DETERMINATION OF THE RATIO OF THE ELECTRO-MAGNETIC TO THE ELECTRO-STATIC UNIT OF ELECTRIC QUANTITY."

By W. E. AYRTON and JOHN PERRY.

INTRODUCTION.

The fact that metals had a different power of conducting electricity was discovered by Sir Humphrey Davy in 1821,* although the idea of resistance as a property of a conductor was not introduced until the publication of Ohm's law in 1827, in which a resistance was first regarded as a magnitude. Now a magnitude necessarily implies a unit of measurement, but the earlier writers merely contented themselves with reducing by calculation the resistance of all parts of a heterogeneous circuit into a given length of some given part of that circuit, so that they generally spoke of the resistance "as the reduced length of the conductor."

The next step was naturally to refer these "reduced lengths" to the length of some standard wire, which might perhaps not be employed in the circuits under test, and to consider the resistance of unit length of this standard wire as the unit resistance. Consequently, we find the unit which was employed by Lenz, in 1838, to be defined as that of 1 foot of No. 11 copper wire, and the unit of Wheatstone, in 1840, as that of 1 foot of copper wire, weighing 100 grains. Until the year 1850 measurements of resistance were confined, with few exceptions, to the laboratory, but about that time underground wires, followed shortly after by submarine

* "*Report to the Royal Society on the New Unit of Electrical Resistance, &c.*" *Professor F. Jenkin.*

cables, began to be employed; and when on these new lines it was no longer possible to determine the position of a fault by inspection, an intimate knowledge of the laws of electricity, combined with an accurate standard of resistance, became of great practical importance to the Telegraph Engineer. The unit of length in the laboratory, "the foot," was replaced in construction by "the mile," thus the unit of resistance in England became that of a mile of No. 16 copper wire, and in France that of a kilometre of iron wire 4 millimetres in diameter. Several other units were from time to time proposed, of which two, that of Weber and that of Thomson, differed altogether from the others in their fundamental conception. Excluding the two last, all the units of resistance hitherto proposed were based on the obstruction offered to an electric current by a given length, of a given material of a given section at a given temperature; but as soon as it had been ascertained that a comparative slight trace of certain impurities introduced into a conductor seriously affected its specific resistance, it became clear that no one of the previously proposed standards was sufficiently definite. Consequently, Jacobi in 1848 felt it necessary to send to Poggendorff and others a certain copper wire, since well known as "Jacobi's standard," in order that electric copies of it might be taken to avoid the growing inconvenience of the multiplicity of standards.

But measurements of resistance can be conceived and carried out entirely without reference to the special qualities of any material whatever, and in 1849 Kirchhoff had effected a measurement of this nature; it was not, however, until 1851 that Weber proposed a distinct system of measurement, based on the fundamental units of length, mass, and time, and such that electrical resistance, according to it, would be expressed by an absolute velocity.

Previous to this, Gauss, desiring to obtain precise measurements of terrestrial magnetism at different parts of the earth's surface, found it necessary at the outset to decide on a unit of force, which was not, like the weight of a pound, affected by the position of the place in which the experiment was made. He therefore devised what has since become well known as Gauss' "absolute" or rela-

tive unit of force based on the fundamental units of length, mass, and time. In accordance with this nomenclature of Gauss', Weber called his method of electrical measurement the "absolute electro-magnetic" system. As soon as the proposal of Weber appeared, Thomson accepted and extended it, showing that the unit of absolute work, the connecting link between all physical forces, formed part of the same system. Consequently, the units of resistance of Weber and Thomson were not based on the physical properties of any special substance, but merely on the fundamental units of length, mass, and time.

Mention must not be omitted of the mercury unit of Siemens, since, although not an absolute one, the coils and apparatus constructed by Dr. Siemens were made with such care that his system has materially helped in obtaining the present accuracy of the standards issued by the Committee of the British Association.

In addition to the vagueness introduced by selecting the resistance of a special rod of some material as our standard, there would be the consequent necessary introduction of various numerical co-efficients into the equations connecting current, resistance, electro-motive force, work, &c. It was, therefore, thought desirable by the Committee of the British Association, when appointed in 1861 to consider the question of the selection of electrical units, that they should decide on some system, not only independent of any particular material, but also of such a nature that every simple equation connecting the absolute measurements of force, work, electric and magnetic quantity, current, resistance, electro-motive force should be independent of numerical co-efficients. But there are, as is well known, six fundamental equations connecting those five electric and magnetic quantities :—

$$1.\ f = \frac{q_1 q_2}{d^2}$$

$$2.\ f = \frac{m_1 m_2}{d^2}$$

$$3.\ q = c\,t$$

$$4.\ f = \frac{c\,l\,m}{d^2}$$

$$5.\ w = c^2\,r\,t$$

$$6.\ c = \frac{e}{r}$$

one equation, in fact, more than is necessary to produce a single system of units. The consequence is, that one or other of the two similar equations, Nos. 1 and 2, connecting electric and magnetic quantity with force, must be rejected. If we reject No. 2, then 1, 3, 4, 5, 6 determine the electro-static units of quantity, current, magnetic pole, resistance and electro-motive force; and by rejecting No. 1, then 2, 4, 3, 5, 6 determine the electro-magnetic units of magnetic pole, current, quantity, resistance, and electro-motive force. The names "electro-static" and "electro-magnetic" refer, of course, only to the fundamental conceptions of the two systems, and do not in any way imply that in these systems the electricity must be at rest or in motion; thus, for example, we may have an electro-static unit of current.

Nature of "v," and the Importance of Measuring its Value.

The object of the investigation described in this paper is to determine the value of "v," the ratio of the electro-magnetic to the electro-static unit of quantity; but it may be asked, since measurements in electro-magnetic units are alone employed by telegraph engineers, of what interest has such an investigation or its result to them? The answer is, we cannot, from the nature of an electro-motive force, have a standard cell of the same constancy as a resistance coil;* therefore, if any one desires to measure with great accuracy the electro-motive force of his battery, he is not able to do this by a simple comparison with a standard cell, but he must determine it absolutely himself. Now, the simplest way to do this is to measure its *electro-static* value with an absolute electrometer, and to convert the result into electro-magnetic measure, or into volts, by using the proper multiplier, which necessarily depends on the ratio v. There are also other practical uses that may be made of this constant, but the main interest attached to the exact

* The electro-motive force of Mr. Latimer Clark's mercurous sulphate cell is undoubtedly very constant, but is necessarily altered by the shaking in travelling, by the presence or absence of free mercury in the paste, by the mode in which the mercurous sulphate is prepared, &c. In fact, as we have pointed out (Proc. Roy. Soc., No. 186, 1878), measurements of electro-motive force are far more delicate than any chemical tests.

determination of its value consists in its constituting a test of the accuracy of the theory that the same medium which transmits the vibrations that constitute light transmits those also which produce electro-magnetic induction.

For the two units of electric quantity are of a totally different nature from one another. If, for example, you take a yard and a foot, two units of length, and divide the one by the other, you get the simple number 3; or if you take two units of weight, a ton and a pound, you get a simple number 2,240; but if you divide the electro-magnetic by the electro-static unit of quantity, it is more like dividing a solid by an area; in that case you do not get for your quotient a number, or a solid, or an area, but a length; so the ratio of the electric units is not a number but a velocity,* and an absolute velocity in nature independent of the units of length and time, and Professor Clerk Maxwell has proved† that this velocity must be that of the propagation of electro magnetic disturbances in a non-conducting medium; or assuming that light is an electro-magnetic disturbance, must be equal to the velocity of light.

I.—Previous Measurements of "v."

The first estimate of the relation between a quantity of electricity measured electrically and the quantity transferred by a current in a given time was made by Faraday;‡ but being measured in arbitrary units, as the absolute system was not then developed, Faraday's comparison gave no indication of the value of v.

There are several ways of measuring this value. The first numerical determination was made in 1856 by Messrs. Weber and Kohlrausch,§ and their method was founded on the measurements of the same quantity of electricity first in electro-static and then

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* The dimensions of a quantity of electricity measured electro-statically are $\left[\frac{L^{\frac{3}{2}}M^{\frac{1}{2}}}{T}\right]$, measured electro-magnetically, $[L^{\frac{1}{2}}M]$; therefore the dimensions of the ratio are $\left[\frac{L}{T}\right]$ a velocity. (Jenkin, "Electricity and Magnetism," p. 164.)

† Electricity and Magnetism, chapter xx.

‡ *Experimental* Researches, Series iii., Section 362.

§ *C. Maxwell*, Electricity and Magnetism, chapter xix., p. 370.

in electro-magnetic units. The result they obtained was 310·7 million metres per second.

But as the quantity measured electro-statically was practically the amount discharged in a finite time by a Leyden jar previously electrified to a fixed difference of potentials, and the amount measured electro-magnetically was the instantaneous discharge of the same Leyden jar electrified to the same difference of potentials, it is probable that the first result and consequent value of v obtained was, on account of the electric absorption of the glass, rather too large.

The next determination of the value of v was made by Sir William Thomson in 1868,* who measured the same electro-motive force electro-statically with his absolute electrometer, and electro-magnetically by determining with an electro-dynamometer the electro-magnetic value of the current sent by this electro-motive force through a known resistance. The mean of eleven sets of experiments from which the highest value obtained was 292, and the lowest 275, gave as a result 282·5 million metres per second.

In the preceding method two forces had to be separately measured, one by means of an electrometer, and the other with the electro-dynamometer; but Professor Clerk Maxwell,† about the same time, carried out a method by means of which these two forces were made to balance one another, so that the ratio of the electro-static and electro-magnetic measures of the same electro-motive force was obtained without previously ascertaining the value of each. The highest of the twelve most accurate results was 294, and the lowest 284, and the mean for v 288 million metres per second. In both these methods it was necessary to know the absolute resistance of a certain coil employed.

It will be observed that even the highest values obtained by either Sir William Thomson or Professor C. Maxwell were lower than the velocity of light, which is about 300 million metres per second, and their mean values were far lower.

In 1872 a redetermination of the value of v was made by Mr.

* Sixth Report of the Committee of the British Association on Electrical Standards, 1869.

† *Philosophical Transactions of the* Royal Society for 1868.

Dugald McKichan, in Sir William Thomson's laboratory,* using the same method that Sir William had previously employed, but, as certain improvements had, since 1867, been introduced into the absolute electrometer, the results now obtained were more accurate. The mean value on this occasion was 293 million metres per second, still, however, being much lower than the velocity of light. It is important, however, to notice that some of the single values obtained were as high as 300 million metres, although these are again balanced by others as low as 290 million metres per second.

II.—Method Employed in the Present Investigation.

Now the velocities ascertained for light are:—

M. Fizeau...	314	Million metres per second.
Aberration, &c., and Sun's Parallax	308	
M. Foucault	298	
M. Cornu	300	

When, therefore, Professor Perry and myself, in 1877, took up the question experimentally, it could not be said that the ascertained value of v was equal to the velocity of light, although Professor Clerk Maxwell's electro-magnetic theory required this identity for its corroboration. In fact, the best experiments seemed to show that v was, for some reason, less than the accepted velocity of light.

The question therefore arose, was Professor Clerk Maxwell's theory incomplete, or was there an error in the accepted velocity of light, or was it that the methods previously employed for the determination of v might be improved on, and a more correct value obtained? This led to the consideration of what other methods than those employed by MM. Weber and Kohlrausch, Sir William Thomson and Professor Clerk Maxwell were available. It was possible to determine its value by an accurate comparison of the electro-static capacity of a condenser with the electro-magnetic capacity of self-induction of a coil,† but it seemed

* *Philosophical* Transactions of the Royal Society, 1873, p. 409.

† *Clerk Maxwell's* Electricity and Magnetism, page 379.

to us very doubtful from the nature of the experiments whether this method would give results more accurate than those previously obtained. And the same remark applied even with greater weight to the measure of a resistance electro-statically and electro-magnetically, since the same difficulty would here have been met with in measuring a resistance electro-statically, that is, encountered when it is desired to measure the insulation of a cable very accurately by loss of charge. It would be possible, by discharging through a delicate tangent galvanometer, or some form of absolute electro-dynamometer, the charge of an air-condenser many times per second, to determine the electro-magnetic value of the current so produced, and as the electro-static value of the current is known from the number of discharges per second, from the geometrical dimensions, and the difference of potentials to which the plates are charged, we should have a fairly accurate means, but one hitherto unemployed, for determining v.

But it appeared, for the following reasons, to Mr. Perry and myself, that the method best suited for the accurate determination of v, was one that had also not been previously employed, and which consisted in measuring the capacity of an air-condenser—(1), electro-magnetically, by the swing of the needle of a ballistic galvanometer, and (2), electro-statically, by the measurement of the linear dimensions of the condenser.

A, because the equation connecting these capacities

$$k = v^2 \mathrm{K}$$

k being the absolute electro-static capacity

K being the absolute electro-magnetic capacity

leads to an equation involving only the *square root* of a resistance, so that if any unknown error existed in our coils, only the *square root* of that error would be introduced into the answer, whereas in all the methods previously employed the error in v was directly proportional to that in the coils.

B, because only one accurate measuring instrument, a delicate *ballistic galvanometer* need to be employed,

whereas, in all the other methods previously employed, two accurate instruments, such as an absolute electrometer and galvanometer, &c., were necessary.

Two difficulties, of course, presented themselves in this investigation, difficulties that it took us many months to overcome, labouring, as we were, under the disadvantage of experimenting in a country like Japan. They were:—

1. To obtain a large air-condenser, of which the plates had sufficiently true surfaces that the electro-static capacity could be accurately measured, at any rate, when the plates were not farther than half a centimetre from one another.

2. To obtain a galvanometric arrangement of sufficient sensibility to measure the small capacity of such an air-condenser, and sufficiently ballistic that the air damping should be almost inappreciable.

III.—The Condenser.

A B C, fig. 1, represents a vertical section of the plane upper brass plate, 1324·96 square centimetres in area, and *D E F G*, fig. 2, the same in plan. The plate is strengthened by stout brass ribs, *D F*, *E G*, fig. 2, and *A B*, *B C*, fig. 1. *L L L* are three chemically cleaned and paraffined ebonite levelling screws, the ends of which are thinned to a blunt point, so as to allow extremely little surface leakage, and by means of which the plate *A B C* can be adjusted parallel to the lower plate *H J K*, fig. 1, which is shown in plan Fig. 3 as *L M N P*. This plate is also strengthened by stout brass ribs underneath *L N*, *M P*, fig. 3, and *H J*, *J K*, fig. 1. This lower plate, by means of hole slot and plane, rests on three chemically cleaned and paraffined ebonite levelling screws, *ll*, by means of which its *upper* surface is made to exactly coincide with the top of the guard ring *Q R*, *S T*, fig. 1, and *U V W X*, fig. 3. This guard ring is rigidly soldered to the upper edges of the brass box *b b b b*, figs. 1 and 3, three projections on the side of which support, with hole slot and plane, the levelling screws *L L L*. Into the bottom of this box screw the

ebonite levelling screws *l l*. Small vessels, containing calcium chloride (not shown in the figure), are placed inside the brass box *b b b b*, to keep the atmosphere in the neighbourhood of the ebonite levelling screws *l l* quite dry, in order to avoid the possibility of surface leakage.

In the earlier experiments the space between the edges *H K* of the lower plate and of the guard ring *R S*, fig. 1, was very small, but afterwards, to avoid the possibility of leakage across by sparking or otherwise, this was enlarged to 2·5 millimetres, and the area of the lower plate thus reduced to 1323·14 square centimetres.

The errors arising from the surfaces of the condenser plates not being true planes, were practically eliminated by capacity experiments being made with successive adjustments of the condenser plates, a different set of points in the upper plate being each time brought to the fixed distance from the lower plate.

IV.—The Galvanometer.

The galvanometer employed was one constructed some time back by Messrs Elliott, from our own design. It had a resistance of 19,970 ohms at 21·4° C. In ordinary use when fitted with an astatic combination, four magnets being used top and bottom, one Daniell's cell would give through a resistance of 600 megohms, a deflection of 130 scale divisions on a scale about 1½ metres distant. But this arrangement, with its aluminium vane, had far too much damping for being used ballistically. We therefore commenced by removing the vane, and weighting the lower set of needles with pieces of brass, so as to give it a barrel shape, but if the brass was light we found there was too much damping, or if heavy, too little sensibility; consequently all the numbers obtained for *v* prior to June 18th, 1878, we rejected. We now built up two little magnetic balls, each consisting of 20 magnets, previously magnetised to saturation and slightly separated from one another with pieces of zinc; in each ball all the magnets pointed one way, and the two balls were used to form the astatic needle. As it would have been too difficult to make this entire sphere all of magnets, we finished it off with segments cut from a wooden sphere.

Now these magnetic spheres gave us an astatic arrangement of considerable sensibility, and without very much damping, the decrement, or the ratio of one swing of the galvanometer needle to the next, being 1·274. About June 18th we made experiments, using this astatic combination, but fearing that even this decrement was too far from unity, we took the needle down in the interval between the 18th and the 23rd, and replaced the segments of the wooden sphere by segments of a small leaden hemispherical *shell*, thus getting a considerable moment of inertia without much extra weight on the fibre. The decrement was now found to be diminished to 1·1695, and with a periodic time for the swing of the needle equal to 39·5 seconds very consistent results were obtained.

It might at first sight appear that the amount of damping action was not very important provided that it was known, seeing that Professor C. Maxwell gives, on p. 348 of his "Electricity and Magnetism," the complete formula for determining the capacity of a condenser by the swing of a galvanometer needle with any amount of damping. In reality, however, this formula is, as explained by Prof. C. Maxwell, developed on the assumption that the resistance of the air is, for slow velocities, directly proportioned to the velocity; but since we know for large velocities it is proportioned to the square, or higher powers, and since the law is not of course discontinuous, the resistance, even for low velocities, cannot be accurately proportional to the velocity, hence the only way to get perfectly correct results is to diminish the retardation arising from the air, or other causes, to nearly nil.

V.—Method of Experimenting.

A current from 382 perfectly new porous-pot Daniell's cells was passed constantly through a resistance $A\ B$, fig. 4, the difference of potentials at two points $A\ C$ was then employed to send a current through the shunted galvanometer, and through a known resistance R, and the deflection obtained was say d_1. Without in any way altering the adjustment of the galvanometer, the connections were then arranged as in fig. 5. By means of the key K the upper plate U of the condenser could be connected either with one pole of the battery or with the other. The fork F, turn-

ing on the pivot P, consisted of two arms perfectly insulated from one another, the one f_1 connected with the point A of the resistance coils, the other arm f_2, which consisted of a piece of Atlantic Cable core with pointed paraffined ends (to prevent any surface leakage), was connected with one terminal of the galvanometer. The stiff wire w, rigidly attached to the lower plate L of the condenser and passing through a hole in the bottom of the brass box $b\ b\ b\ b$ without touching it, could, therefore, by turning the fork F, either (1) be connected with the pole A of the battery, or (2) left insulated, or (3) discharged through the galvanometer. Both surfaces of contact were platinised. (The same sets of connections might have been arranged with an ordinary "charge and discharge key," but with not such perfect absence of leakage, for the lever of such a key, which is supported on ebonite pillars, and along which some surface leakage must have taken place, would have had to be connected with the wire w.) The box and the other pole of the galvanometer were permanently connected with A, which was joined to earth.

A complete experiment was as follows:—

1. f_1 pressed against w, and k pressed down, so that v (fig. 5) was connected with B.
2. f_1 removed from w, and then k liberated, so that v was discharged.
3. f_2 pressed against w, so that L was discharged through the unshunted galvanometer producing a deflection d_2.

The *rationale* of the process will easily be seen. By making L part of the brass box while charging, we are independent of the action of its edges, and of the shape of the curved ribs on its lower surface, see figs. 1 and 3; and by connecting U with the box before discharging L, we obtain a complete discharge from the latter.

The experiment was occasionally varied by leaving L insulated for *some time* after putting U to earth, and the apparatus was not considered in good order if any perceptible diminution from leakage in the subsequent discharge was observed to result from an insulation of *several seconds.*

Let C be the current in absolute electro-magnetic units (gramme, centimetre, second) which flows in the first case through the galvanometer,

a_1 the angular deflection produced,

g the resistance, in absolute units, of the galvanometer,

s the resistance, in absolute units, of the shunt,

R the resistance, in absolute units, introduced into the circuit,

G* the magnetic galvanometer constant,

H the horizontal intensity of the uniform magnetic field in which the needle moves;

then $$C = \frac{H}{G} \tan a, \text{ approximately.}$$

Let V be the difference of potentials maintained by the battery at the points A and B, then

$$C = \frac{s}{s+g} \cdot \frac{r_1}{r_1 + r_2} \cdot \frac{V}{R + \frac{s\,g}{s+g}}$$

$$= \frac{r_1}{r_1 + r_2} \cdot \frac{s\,V}{(s+g)\,R + s\,g}$$

Let K be the capacity of the air condenser in absolute electro-magnetic units (gramme, centimetre, second), then if a_2 is the angular swing produced by the discharge in the second case, and P the periodic time, in seconds, of the needle swinging freely,

$$V\,K = \frac{H\,P}{\pi\,G} \sin. \frac{a_2}{2}$$

$$\therefore \quad K = \frac{P}{\pi} \frac{r_1}{r_1 + r_2} \frac{s}{(s+g)\,R + s\,g} \frac{\sin. \frac{a_2}{2}}{\tan a_1}$$

$$= \frac{P}{\pi} \frac{r_1}{r_1 + r_2} \frac{s}{(s+g)\,R + s\,g} \frac{d_2}{2\,d_1} \text{ approximately,}$$

a result quite independent of the electromotive force or resistance of the battery.

If A is the area of the lower plate of the condenser in square centimetres, t the distance, in centimetres, between the plates, k

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* G is defined such that a current of strength C produces on a needle, of *magnetic moment* M, when parallel to the galvanometer coils, a couple of *which the moment* is C M G.

the electro-static capacity in absolute units (gramme, centimetre, second),

$$k = \frac{A}{4\pi t}$$

but $$k = v^2 K$$

$$\therefore \quad v = \sqrt{\frac{A}{4\pi t}\,\frac{\pi}{P}\cdot\frac{r_1 + r_2}{r_1}\cdot\frac{(s+g)\,R + sg}{s}\cdot\frac{2\,d_1}{d_2}};$$

d_2 is supposed to be the *undamped* deflection of the galvanometer, but, as there was always some slight damping even in our *ballistic* galvanometer, the following correction must be introduced :—for d_2 we must write

$$(1 + \tfrac{1}{2}\lambda)\,d_2$$

where λ is the logarithm to the base e, or $2{\cdot}7\dot{1}82\dot{8}$, of the decrement.

If, now, all the resistances be measured in ohms, the complete expression for v becomes

$$v = \sqrt{\frac{A}{4\pi t}\cdot\frac{\pi}{P}\,10^9\,\frac{r_1 + r_2}{r_1}\cdot\frac{(s+g)\,R + sg}{s}\cdot\frac{2\,d_1}{(1 + \frac{1}{2}\lambda)\,d_2}} \cdot (1)$$

In actual practice, of course, the mean of a large number of discharges of the air-condenser was employed, and great care had to be taken that the needle was absolutely at rest before each discharge, since, with such a large moment of inertia, an extremely small angular velocity means a considerable angular momentum, and consequently a considerable error if disregarded. Consequently, even when a number of weak auxiliary checking currents were employed to stop the needle (when swinging after a discharge), considerable delay had always to occur between two successive discharges.

June 18*th*, 1878.

$A =$	$1324{\cdot}96$	square centimetres.	
$t =$	$1{\cdot}024$	centimetres.	
$P =$	$25{\cdot}3$	seconds.	
$r_1 =$	$3{\cdot}0045$	ohms.	
$r_1 + r_2 =$	8538	,,	
$R =$	12000	,,	
$\frac{s}{s+g} =$	$\frac{1}{1000}$		
$d_1 =$	$297{\cdot}34$	scale divisions.	
$d_2 =$	$261{\cdot}63$	,, ,,	mean of 39 discharges.
$\frac{1}{2}\lambda =$	$0{\cdot}12095$		

Weight of the needle complete, with the forty magnets, the wooden segmental pieces, and the mirror, 2·15 grammes.

Equation (1) gives $v = 297{\cdot}4$ million metres per second.

June 23rd.

$A =$	1323·14	square centimetres.	
$t =$	0·7728	centimetres.	
$P =$	39·5	seconds.	
$r_1 =$	3·0045	ohms.	
$r_1 + r_2 =$	10037·16	,,	
$R =$	12000	,,	
$s =$	19·955	,,	
$g =$	19733	,,	
$d_1 =$	247·75	scale divisions.	
$d_2 =$	221·93	,, ,,	mean of 41 discharges.
$\frac{1}{2}\lambda =$	0·07825		

Weight of the needle complete, with the forty magnets, the small segments cut from the leaden sphere, and the mirror, 3·4 grammes.

Equation (1) gives $v = 299{\cdot}5$ million metres per second.

June 25th.

$A =$	1323·14	square centimetres.	
$t =$	0·7728	centimetres.	
$P =$	42·2	seconds.	
$r_1 =$	3·0045	ohms.	
$r_1 + r_2 =$	10040	,,	
$R =$	12000	,,	
$\frac{s}{s+g} =$	$\frac{1}{1000}$		
$d_1 =$	263	scale divisions.	
$d_2 =$	223·9	,, ,,	mean of 18 discharges.
$\frac{1}{2}\lambda =$	0·081865		

Weight of the needle complete, with the forty magnets, the small segments cut from the leaden sphere, and the mirror, 3·4 grammes.

Equation (1) gives $v = 297{\cdot}2$ million metres per second.

Mean of the three values of v, *i.e.*, the final result from the ninety-eight, discharges of the air condenser is

298 million metres per second,

or, *exactly the velocity found by M. Foucault for light.*

The probable error of our answer 298 is about one per cent., but the difference between M. Foucault's velocity for light, 298 million metres per second, and M. Cornu's, 300 million metres per second, is even less than one per cent. We may therefore now conclude that these two velocities for light, as well as the value obtained by the method we have employed, and which theoretically ought to give the most accurate determination, for the ratio of the electro-magnetic to the electro-static unit of electric quantity are all equal within the limits of even our experiments.

The PRESIDENT: I am sure we are much obliged to our friend Prof. Ayrton for his paper, but it is one which I think cannot be discussed without the opportunity of studying it in print. I will, therefore, call upon Mr. E. A. Cowper to read his paper.

THE WRITING TELEGRAPH.

BY E. A. COWPER, M.I.C.E.

I shall not have to trouble you with any mathematics this evening, but simply a few mechanical problems; indeed, I have had but a very simple task myself, to calculate out and proportion a few resistances, and also to proportion a few separate parts of the machine to their work.

Now, in order to render intelligible, in the fewest words, my description of the instrument, I must ask you to bear in mind that, contrary to the usual practice, I do not change my currents from positive to negative at any time, neither do I ever send sudden shocks or charges through the wires, nor do I spell the letters, or decompose them into so many separate signals of "dots" and "dashes," much less do I step all round a dial to get at the letter I want, or to print from a type that is not in the printing position.

But you will at once appreciate the theory on which this instrument is constructed, if you will just consider for a minute (as I did myself when I worked out this idea about two years ago) what are the actual elements of a curve, and you will see that, inasmuch as we can find any one spot, or any number of spots on the surface of the globe by simply giving the latitude and longitude of the spot, and can continue to trace a ship's course over the ocean by continually giving the latitude and longitude, so by decomposing, so to speak, or resolving into its elements, a piece of writing, we could in fact set out or draw the writing again from such information; now this is exactly what the instrument before you really does in practice.

First, then, to obtain the information to be transmitted, we must in some way take into account, or gauge the varying positions of the pencil in the hand of the operator, both vertically and horizontally, and then use the gaugings so obtained to produce like results at the other end of the line, so that the pen at the receiving end of the line shall move absolutely and instantaneously with the pencil in the hand of the operator.

Secondly, we come to the mechanical mode of so appreciating the information we want, and to this end there are two little "connecting rods," or "contact rods," jointed to the pencil of the operator, and one of them passes upwards and the other horizontally, so that the vertical "contact rod" slides up and down, exactly in accordance with the vertical motion of the pencil, whilst the other is totally unaffected by such motion, and then again the horizontal "contact rod" slides along from right to left, or left to right, exactly in accordance with the motion of the pencil horizontally, and without interfering with the vertical "contact rod:" we therefore now have two measuring rods, if I may so call them, which have thus measured, decomposed, or separated into its elements every line made by the pencil, be it straight, slanting, or curved, or whether the figure drawn be triangular, round, or square; so that we have now only two simple straight line motions to be transmitted to the other end of the line.

Thirdly. The better to illustrate this important fact on which *the telegraph* instrument is based, I have here a diagram model

of two cams, as an illustration of curves resolved into their elements, which simply give, the one an up and down motion to the pencil, and the other a right and left motion to the pencil; this simple diagram has nothing to do with the telegraph instrument, but as I know from experience that one cannot always at a glance fully believe and feel that two simple motions, at right angles to each other, will produce any curve, perhaps I may be excused for showing to demonstration that it is so.

(The author pointed out that one cam gives vertical motion to the pencil, and the other horizontal motion to it, so that it writes the letters W.T.) In this model the curves were resolved into simple motions, and the motions reproduce the curves.

Fourthly. We come to the use made of the elementary motions of the two "contact rods" in the telegraph instrument. The vertical "contact rod" touches and slides over a series of metal "contact plates," placed very close together, but insulated from each other, the highest one being in direct communication with a line wire, whilst the next plate is connected to the first, through the intervention of a resistance coil of definite resistance, the third "contact plate" is connected to the second through another resistance coil, and so on through the whole 32 in number, the resistances being properly proportioned so that the weakest current is sent when the "contact rod" is lowest, and in contact with the lowest plate of the series, and the strongest current is sent when the "contact rod" is highest, and in contact with the highest plate of the series.

Fifthly. The horizontal "contact rod" slides over a similar series of "contact plates," arranged very nearly in the same way, and thus the current sent is either strong or weak, according to the position of the pencil horizontally, there being two line wires, one for transmitting the current for vertical motion, and the other the current for horizontal motion. Before leaving the transmitting instrument, I should explain that instead of the operator moving his hand and arm across the paper when writing, he keeps his hand in one place, and writes within a small but ample space before him, the paper moving away at a moderate speed as he writes on it, so that he *can see what he is* writing and has written, and this

paper is the record of what has been sent; the paper delivering mechanism, or clockwork, being used for the receiving instrument at the station, as well as for the transmitting instrument at the same place.

I am fully aware that I shall probably here be met with the observation that two wires cannot be afforded in some cases; but I may say that the chief application of this instrument will probably be for lines of moderate length, where the cost of two wires instead of one will be a matter of small moment; for instance, such lines as private telegraphs, and telegraphs in all large cities and towns, with their suburbs, though longer lines will by no means be left out of sight.

My opinion, that two wires can in such cases generally be afforded, is confirmed by that of experienced electricians.

At present only those results which have been actually obtained are brought forward, and no increase whatever has been made to a very usual amount of battery power, viz., 20 cells of Muirhead's (Daniell's) battery, charged quite weakly.

The resistance that the first instrument has been writing through since its birth (not two months ago) is equal to 141 ohms, or, I suppose I may say a resistance equal to about 8·3 miles of land wire, or 40 miles of submarine cable. But the instrument now before you works through 400 ohms, or, say, 25 miles of land wire, or 120 miles of cable.

Of course it is obvious that if the conductor was increased in size somewhat, or the battery power increased, there would be more margin available for working this instrument, and much longer lines might be worked than have been attempted at present; and if both were increased, very long lines might be worked.

Sixthly. We now come to the "receiving instrument," and here precisely the reverse operation is carried on to that which took place at the "transmitting instrument," for the two variable currents, representing the elementary motions of the curves, are utilised to again form the very curves which originated them—that is to say, the current from the "contact plates," over which the *vertical* "contact rod" works, is caused to move a needle to give *vertical motion* to the writing pen (by a rigid or flexible connection),

and in just the true proportion to the vertical motion of the pencil in the hand of the operator at the transmitting end of the line; and so in like manner the current from the "contact plates," over which the horizontal "contact rod" works, is caused to work another needle, to give horizontal motion to the same writing pen, and in true proportion to the motion of the pencil in the hand of the operator at the transmitting end of the line.

The stationary electro-magnets are laminated, being made of very thin iron plates, such as I employed in an electro-magnetic machine used by my father to spin cotton on the lecture table at the Royal Institution in 1838, as such plates almost entirely do away with residuary magnetism.

Therefore, if the writing pen moves up and down just as much, and at the same times, as the pencil of the operator, and also just as much horizontally, and at similar times, it follows that it makes the same lines, or in other words writes just as well as though the operator had the writing pen in his hand; in fact, he writes in pencil at the transmitting end of the line, and the pen writes in ink at the other. The pen or writing instrument may be of any kind that will write with but little friction, the one used in the instrument is the old syphon pen made of a fine tube, like that used by Leverett Bradley in 1859 for a telegraph, giving zig-zag indications on paper as the paper was passed through the instrument.

The paper delivering mechanism or clockwork draws the paper out of the instrument.

The message written in ink is thus continually run out of the machine, as long as any one writes at the other end, and no receiving clerk is required to watch a dial or needles, or to listen to sounds, the work being done by the instrument entirely without him, and each message of course being separated by distinct writing from the next, though all are on one long strip of paper, and each only requires to be cut off and put in an envelope and sent to its destination; all time and labour of writing out the message at the receiving end being saved, and all chance of misunderstanding between the operators, for want of attention to the usual signals, being done away with at once. To save directing an

envelope, the telegram may be wound on to a card with the address outwards.

The two needles in the receiving instrument are, of course, placed at right angles to one another, as shown in the model and in the instrument itself, and they in no way interfere with each other; the precise mode in which they are made to take up their proper positions at all times is that they each pull against a spring, so that they overcome the spring more when the current is strong than when it is weak, so that the motion of the pen is absolutely governed by the strength of the current sent. As some slight proof of the definite manner in which the contact plates and resistance coils act, a galvanometer in the circuit (when increasing the strength and when decreasing), showed very little difference between the readings.

The currents being continuous in one direction, though constantly varying in strength, the change from a weaker current to a stronger, or *vice versa*, does not amount to emptying the line wire and recharging it, but simply to an extra push, so to speak, or a reduction of force, as the case may be, and even if the needle should be a little slow in following every curve no doubt it will follow and make its proper mark; indeed, except in experiments, or when some part was altogether out of adjustment, the pen has behaved remarkably well.

It may be mentioned that a few days after the instrument started writing on the 2nd January, it was noticed that it did not make the tails to the y's and g's long enough, but a few yards more of resistance coil added to the lower contact plates pulled down all the tails of the letters in a most satisfactory manner, just showing how thoroughly amenable the instrument was to proper treatment.

With regard to the speed of the instrument, it is of course limited for common writing, to the rate at which such writing can be written, say 40 words per minute or thereabouts; but the instrument is not yet nearly up to that speed, though it has been improved week by week, and the instrument now before you, which is only the second one which has been made, (and is really quite a rough one), is largely in advance of the first (its adjustment of

resistances is not yet complete), and there is no doubt but that, when made in a better manner, and with a little more experience, still better results than those brought before you will be obtained.

If, however, the very earliest results thus brought before the members, at the request of the Council, have been of interest to them, as showing how a certain long wished-for object can be obtained by a little simple mechanism specially adapted to the purpose, the author will be more than rewarded for not waiting until he could show a more perfect instrument.

The PRESIDENT: Gentlemen, it is rather late, but I am sure you will allow me to express in your name the heartfelt thanks of the Society to Mr. Cowper for the very graphic account he has given us of a most ingenious idea. There is no doubt we are all very gratified, and I think he may congratulate himself upon the great perfection of the instrument he has constructed and exhibited to-night for our benefit, considering the short time he has had to get it ready. I am sure I may also tell him in your name that we shall be exceedingly grateful to him if he will be good enough to communicate to the Society the results of any further development of his ideas in this direction, and how he has succeeded in overcoming any little difficulties which stand in the way of bringing his instrument to that complete perfection which he desires it to attain. In the meantime, I am sure we all wish him hearty success.

The meeting then adjourned.

ORIGINAL COMMUNICATIONS.

ON MAGNETISING IRON WHILE CASTING.

By E. Chernoff.

Communicated by Dr. C. W. Siemens, F.R.S.

Sir,

About three weeks ago I made an experiment very interesting in a scientific point of view, and hasten to communicate it to you.

Commencing with the following propositions:—

1. Hardened and magnetised steel loses its magnetism when heated to redness.

2. The harder the steel is tempered the more permanent the magnet, but the more difficult is it to magnetise.

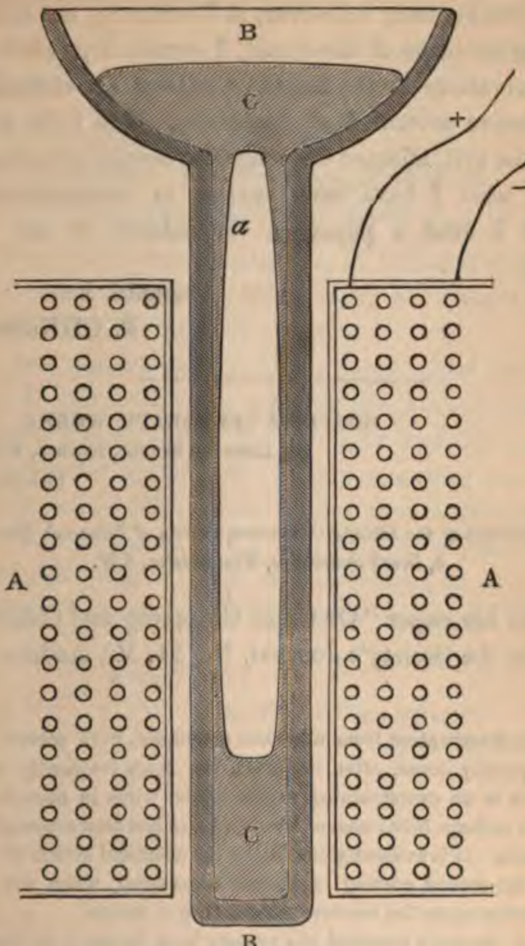
3. White cast iron presents the highest degree of hardness or temper, a degree it is almost impossible to communicate to steel, and therefore if it were possible to magnetise white cast iron, it would become the most permanent magnet.

4. That the hardening of white cast iron takes place at its cooling from the melted state.

I cast some white refined iron in a mould, surrounded by an electro-magnetic reel, along which a current was allowed to flow during the process of casting, so that the fluid metal became magnetic, and cooled under the influence of the magnetic current. I was not wrong in my expectations, for I obtained a magnetised bar of white cast iron.

I noticed the following phenomena. While pouring the metal into the mould, I observed an agitation of the surface until the metal set; this could not have proceeded from damp, as the mould was thoroughly dry. When cool I took the little bar out of the mould, it broke very easily, and proved to be hollow. The

annexed sketch, two-thirds full size, represents a vertical section of the apparatus.



A A, the electro-magnetic reel.

B B, a turned steel mould and funnel.

C C, the casting.

It will be seen by the sketch that the casting proved hollow, and that the thinnest metal was just opposite the centre of the reel, and the thickness there did not exceed that of writing paper. It is evident *that the fluid metal* became magnetic, and that its

particles were repelled towards the poles of the magnet, which caused the agitation observed in cooling. I found that the point *a* was the most strongly magnetic; but as the thin portions of the casting broke, and, moreover, as I broke up the lower end to examine the structure of the metal, I cannot at present make any further observations on the magnetic state of the casting, but will have to observe several fresh specimens. The little pipe, when cleared of the grit, affected the magnetic needle powerfully.

This is what I have been anxious to communicate to you. Last week I read a paper on the subject at our Technical Institution.

I remain, &c.,

E. CHERNOFF.

HOOPER'S TELEGRAPH WORKS,
31, Lombard Street, London, E.C.,
17th March, 1879.

The Chairman of the Editing Committee, Society of Telegraph Engineers,
4, *Broad Sanctuary, Westminster, S.W.*

Sir,—In his paper "On Cable Grappling and Cable Lifting," published in the Society's Journal, No. 24, Mr. Andrew Jamieson says:—

> "Cases of interruption from a broken conductor, with perfect or good insulation, frequently occur, often in joints, but more frequently in the main conductor, due to an overstraining of the copper wires in manufacture, or in paying out, or perhaps brittleness of the copper, as has been reported in the case of Hooper's core. It is evidently caused by the chemical action of the sulphur inherent in the second coating of Hooper's material, when not thoroughly eliminated, acting upon the conductor rendering it brittle. . . . .
> In the case of Hooper's material the remedy is to be found in eliminating, or so insulating the sulphur that it cannot act upon the conductor."

I beg to state that no cases of brittleness of wire have arisen from any chemical action of the sulphur used in the preparation of Hooper's core.

At first sight, Mr. Jamieson's remarks appear to be based upon specific chemical tests, but upon a second reading it appears that *he only speaks* from report.

He moreover does not appear to be fully informed as to the composition of Hooper's core, as he refers to the

> "Action of sulphur inherent in the second coating of Hooper's material."

No sulphur is inherent in the second coating. The three coatings of Hooper's core are :—

1st. Pure rubber.
2nd. "Separator," which contains *no* sulphur.
3rd. "Jacket," which contains sulphur.

The duty of the second coating is, as its name implies, that of separating the vulcanised india rubber, or "jacket," from the india rubber next the copper conductor.

It is a fact that where such action takes place upon copper as changes its ductile condition to a brittle one, a proportionate change takes place in the resistance of the copper, and this change would be at once detected by the tests of the core during manufacture.

No such change takes place in the manufacture of Hooper's core, and this statement will, I am sure, be vouched for by the many electricians who have tested it for years past.

Asking you to be so good as to insert this letter in your next edition,

I am, Sir,

Your obedient servant,

JNO. P. HOOPER.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

Vol. VIII. 1879. No. 27.

The Seventy-fifth Ordinary General Meeting of the Society was held on Wednesday Evening, March 12th, 1879, at the Institution of Civil Engineers, 25, Great George Street, Westminster—the President, Lieut.-Col. Bateman Champain, in the Chair.

The President: Gentlemen,—Before proceeding to the ordinary business of the evening, I must ask leave to say one or two words in explanation of a remark made by me at the end of my presidential address, which I find has been a little misunderstood. While acknowledging the cordial hospitality displayed (among other places) at Naples to the delegates of the International Congress, I ventured to refer, in rather a jocular way, to certain doubts which had been expressed as to the authenticity of the antiques dug up in our presence at Pompeii. I have this day received a note from my friend, Signor d'Amico, telling me that what I said had annoyed the Senator Fisrelli, the distinguished gentleman who superintends the excavations. Senator Fisrelli is anxious to point out that our good fortune on the occasion in question was absolutely genuine. I need hardly assure you, gentlemen, that I had not the smallest intention of hurting any one's feelings by my casual observation, and I beg you will allow me to retract it unreservedly.

We will now continue the discussion on Mr. Willoughby Smith's and Mr. Graves' papers read a few meetings ago. The discussion has been a little prolonged, and we have several subjects in hand which we cannot well postpone; so that if any gentlemen desire

to offer any observations before Mr. Willoughby Smith replies, I must ask them to be as short as possible. We have received a communication from Mr. Graves, which extends to a considerable length, and we shall not have time to read it, but it must be taken as read, and it will be found printed in the Journal.

Mr. WILLOUGHBY SMITH: Mr. Varley says my diagram marked 2 is incorrect. Mr. Varley was in possession of a printed copy of my paper, and before making such a statement ought to have compared it with the diagram in his specification of the patent to which that diagram refers. I have done so, and find it is a correct copy. But the whole thing is so unimportant that I will not detain you further on that subject, for there is no longer any fear that the high battery power contemplated by Mr. Varley will ever be used for working submarine lines, however great their length.

Mr. Varley says diagrams 9 and 10 are incorrect, because they do not show the whole testing arrangement. My paper treated of the working and not the testing of cables, consequently the introduction of the whole testing system would have only complicated matters, and that was what I wished to avoid, and thus all apparatus not connected with the speaking arrangement was intentionally omitted from the diagrams.

In referring to diagram 3, Mr. Varley appeared to me to be under the impression that I had claimed the discovery of what is known as the "Coil Current," or "False Discharge." That, certainly, was not my intention; neither do I think what I have stated in my paper will bear that interpretation. I am fully aware that Mr. F. C. Webb was the first to notice that phenomenon, and that he published an account of it in 1859. In 1862 the subject was being discussed in the "Electrician." Sir Charles Wheatstone and other gentlemen attributed it to the polarization of the earth plates, or to the imperfect insulation of the wire under experiment. I published the results of my experiments, because one gentleman questioned whether Sir Charles ever had sufficient length of cable under his control to produce the effect, implying that a long length of cable was necessary. I showed that that was not the case, and *that* the results were much more marked when the insulated wire *was wound on* an iron reel.

In addition to the remarks we have already heard from Mr. Varley, he has handed to the Secretary twenty-four sheets of foolscap, closely written over. Since our last meeting, I have had an opportunity of looking over this written statement, and I have no hesitation in saying that some of the paragraphs are written at random. Referring to the meeting which took place on board the *Great Eastern* on the 6th July, 1865, to witness the working of the curb key, Mr. Varley states that "with the curb key and *condenser*, a speed varying from $5\frac{1}{4}$ to $5\frac{3}{4}$ words a minute was obtained."

I am convinced in my own mind that no *condenser* was used or mentioned in 1865 in connection with the actual working of cables.

On the day named, the connections were as I have described them in my paper, the correctness of which Mr. Varley has not challenged.

It would be encroaching too much on your time and patience were I to detain you while I referred to all the erroneous statements made by Mr. Varley; but there are a few, of which I hope you will allow me to treat somewhat in detail.

Referring to the system adopted in testing and working the 1865 and 1866 Atlantic Cables while being laid, Mr. Varley states:

"With reference to keeping up a continuous test for insulation through a cable, the first person to realize this was Sir William Thomson. Thomson, W. Smith, De Sauty, and I discussed this subject on board the *Great Eastern* when returning from the unsuccessful trip. Thomson hit upon the plan for keeping up the insulation test and signalling to the shore by means of his Quadrant Electrometer. I then pointed out how a *Condenser* would aid the speed of working. On arriving in England, Sir W. Thomson sent in his proposition to the Atlantic Telegraph Company, who published it."

What inconsistency! If Mr. Varley used a condenser, as he states he did in July, what need in August to point out how a condenser would aid the speed of working? But the following extracts from my journal will, I think, make all clear; and if so, it will then be seen that Mr. Varley is wrong in stating that he used, or even *suggested the use of a condenser* in 1865, for working

"Wednesday, May 2, 1866.—Mr. Laws informed me this morning that Mr. Clark had requested him to make an appointment with me to test the cable of last year now on board the *Great Eastern*, as Mr. C. wished to certify to the electrical condition of the same on Friday next. Agreed to leave by a late train this afternoon.

Thursday, May 3.—Mr. Laws tested the cable this morning; after which we made some very good experiments between the ship and shore. Mr. L. Clark present part of the day. Busy chief part of the morning endeavouring to fix condensers for speaking from shore to ship, instead of resistance coils; Mr. Laws suggested that if *condensers* were used the tension of the line might always be known by discharging the condenser through a galvanometer.

Thomson and Varley arrived late in the evening.

Friday, May 4.—Thomson and Varley experimenting with their instruments on shore all the morning. Did not get any signals from them, as Thomson had left 80,000 units in circuit on board. On their return to the ship Thomson said his plan worked first rate, but Varley had suggested a much better plan by using condensers.

Considering the condenser and connections were left on shore, the same as we used them on Thursday, I could not give Varley the same credit for his new suggestion as Thomson was inclined to do. Varley and Laws went on shore in the afternoon, and Thomson remained on board, but they could get no reliable results."

Now, with these facts before me, I think I was perfectly justified in my statement that Mr. Varley had no idea of using condensers in connection with the working of the Atlantic Cables until I had already applied them for that purpose.

And in a separate paragraph Mr. Varley states: "The method adopted was mine and Thomson's in every respect, and not that of Mr. W. Smith."

Now I will briefly give the facts of the case and leave it for you, gentlemen, to form your own opinions on the questions at issue.

In the 1865 Expedition I had not the charge of the electrical department, nor was I in any way responsible for the system then used. But, of course, I took great interest especially in the electrical department, and soon saw the great advantage there

would be in keeping up a continuous test, and still being able to freely communicate between ship and the shore. It was not long before I saw a way to accomplish my object. I made no secret of it, and when explaining it to Sir William Thomson I remember distinctly his saying, "Why not use an Electrometer instead of a high resistance?" On the return of the *Great Eastern* I at once set to work, and soon perfected a system which I first connected up at our Wharf Road Works, and showed to my friends as occasion offered.

In January 1866, Professor Jenkin was preparing to give a series of lectures on Submarine Telegraphy at the Society of Arts, and had asked us to furnish him with the amount of stretch and breaking strain of various sized cores. On the 25th of January Mr. Jenkin came to Wharf Road to witness these experiments, and I then showed him the practical working of my system, with which he appeared much pleased, and considered quite new.

On Thursday, February 15th, I find the following entry in my journal:—"Called on Mr Jenkin by appointment at 6, Duke Street. Promised to lend him for his lecture: a tank, my condenser, copper of high and low conductivity, 50 cells and resistance coils. Gave him permission to illustrate my new system of testing. Informed me that he wrote to Professor Thomson on the day I explained my system to him, since which time he had had several letters from him, in one of which he claimed the idea of keeping the ship in communication with the shore as his."

On the Monday following I received a note from Mr. Jenkin as follows:—

6, Duke Street, Adelphi, *February 17th*, 1866.

Dear Sir,—I think the enclosed letter which I got from Thomson this morning will be satisfactory to you. Send it back to me if you please.

Yours very truly,

FLEEMING JENKIN.

I returned the letter, after having taken a copy of the same, which is as follows:—

Physical Laboratory, G.C., *February 16th*, 1866.

Dear Jenkin,—Very many thanks for your letter about Wy. Smith. I am quite taken by surprise with the ingenuity of his plan. I knew he was a *good man* (in the Cambridge sense of the term, or *virtuous* as they have it in Latin), but was scarcely prepared for so thoroughly original a plan as you

describe. I thoroughly admit that he had no assistance or shadow of an idea towards it from me, and I wish you would tell him so FORTHWITH as a message from me. I feel pained at being for even a day supposed by him or any one else to have made a claim, either privately or publicly, of something belonging to another. I well remember saying "Why not use an Electrometer, &c.," as Wy. Smith reports, but I had forgotten that Wy. Smith had been saying he could show how to talk and test insulation simultaneously. I almost think I do recollect now his having said something to this effect; at all events it is quite clear that he had the idea of keeping up the insulation test while receiving signals in the shore station before I spoke of it in the *Great Eastern*. So his describing his plan or original idea to Saunders, and then working it out himself is altogether, both as to invention and priority of publication, his own.

I imagined that Smith's plan, the existence of which I had heard of from Field and Canning and you, was either identical with mine, or so nearly so as to require an Electrometer on shore. I never said a word, however, of any claim of mine to any part of Smith's plan to any of them but you.

The use of the Electrometer for the purpose I propose, I suppose any one will admit to be mine. Yours truly, W. T.

And in a foot note:—

I had myself the idea of using Electrometer for signals on shore during the ship's insulation test long before, but I don't remember having spoken of it.

In addition to this letter of Sir William's, I find the following memorandum:—

"Mr. Varley, speaking of the Atlantic Cable in Liverpool on the 14th of March, 1866, and reported in *The Daily Courier* of the 15th, spoke as follows, concerning my new system of testing.

'Mr. Willoughby Smith had made a most excellent suggestion, by which the double operation of testing the continuity of the cable and its insulation could be made at the same time. Although the two operations would be performed at the same time, the apparatus used would be vastly simpler in construction and manipulation than anything ever tried before, in fact the ship would never be left without the knowledge being obtained on board ship and on shore every moment of the whole time that the cable was continuous and insulated throughout. That was the greatest safeguard that had ever been devised in cable laying, and the greatest credit was due to Mr. Willoughby Smith for that most exceedingly simple and most effective invention.'

Mr. Varley's statement that "The condenser, keys and battery *used were* made by myself and were joined up by myself at

Valentia," and that "The reflecting galvanometer used at Valentia was prepared by Sir William Thomson and myself, and were our property," were so totally at variance with what I believed to be the facts of the case that I wrote to Mr. May, whom I left in charge at Valentia, asking him if there really was anything done at the station after my departure which would warrant Mr. Varley in making such statements. Mr. May replied: "With the exception of the condenser used for speaking, all instruments, batteries, &c., used for your system at Valentia during the laying of the 1866 Atlantic Cable were the property of the Telegraph Construction and Maintenance Company, and were joined up by myself and staff according to your printed instructions and diagram. All alterations required to any of the galvanometers in use were made by Mr. Weatherall and myself. Not being able to receive well on the ordinary mirror galvanometer, I, as you are aware, made use of our marine mirror galvanometer for speaking, which answered well, and was used throughout the expedition. At the commencement of laying the cable Mr. Varley attempted to interfere with our arrangements. I protested against this, and Mr. Glass instructed me not to allow Mr. Varley or any one to interfere with your paying out system."

I consented to take charge of the electrical department while laying the 1866, and recovering the 1865 cables, with the clear understanding that my system pure and simple was to be used, and that there was to be no divided responsibility or interference.

If anything had gone wrong I, and I alone, would have been responsible.

But I am happy in the knowledge that by the hearty co-operation of the gentlemen who assisted me the whole system proved a perfect success.

In Mr. Varley's patent of 1862, to which diagrams 4, 5, 6, 7, and 8 refer, it will be seen that he has claimed the application of condensers, resistance coils, and induction coils connected in every conceivable way. Now, I have no hesitation in stating that that patent belongs to the category designated "Fishing patents," for I am not aware, even at the present time, of any system by which the addition of a resistance coil or induction coil, either separately

or combined, would improve the working of long submarine cables. But suppose any one of you gentlemen were to hit upon a plan by which they might be advantageously used, and after a large consumption of midnight oil had perfected your plans and published them, I have no doubt but that Mr. Varley would at once claim it as his patent of 1862, the same as I contend he has done in the case of the condenser as soon as he found it was being used on the Atlantic Cable in 1866.

Mr. Varley says I am not correct in stating that, if in the present system of working long cables the resistance coil was placed as patented by Mr. Varley, and shown in diagram 4, the speed would be reduced. Now, let us dwell a few moments on this question. Please consider the two cylinders marked 3, and lettered respectively B and A in diagram 13, to represent the condenser at the receiving end, and the battery disconnected from cylinder A, and the cylinder connected to the end of the line marked A in diagram 14. The lines C and D show the boundary of the potential of the cable, on immediate contact; but of course if the battery contact be prolonged a sufficient time the potential of the cable would become the same throughout its entire length, and C would be a straight line, parallel with the line A B. Now, I have shown that on immediate contact with the battery the tension at the far end of the cable is very small indeed compared with that of the battery end, but nevertheless just sufficient to affect the sensitive galvanometer attached to the cylinder B, as I have described. Now, suppose a resistance coil attached according to Mr. Varley's patent, what would be the result? The tension would not be sufficient in the same time, consequently the galvanometer would not be so quickly affected, and, therefore, there would be a diminution of speed. It has been proved to be so in actual practice, but, of course, to what extent depends on the resistance of the coil employed.

From the results of the experiments of Messrs. Thomson and Varley with the curb key in 1865, Mr. Varley says they would have obtained about eleven words per minute through 1900 miles of the cable when laid. The length of the cable on board the *Great Eastern* was 2,273 miles, and not 2,500 as taken by Mr. Varley;

that would alter his figures, but I question whether they would have been able to have obtained anything like that speed in actual practice, for reasons which will be apparent presently.

Mr. Varley has given a very full and interesting account of the nature and application of what are termed Curb Currents, but Mr. Varley cannot alter facts. I say there would be a diminution in the speed were the curb key employed, and Mr. Graves in his interesting paper, which you have heard read, confirms my statement. I do not dispute the fact that the results obtained by Mr. Varley on his artificial line at his own house might have been more favourable to his curb system. I have an artificial line which I believe is more reliable than Mr. Varley's, although I do not expect that gentleman will admit it; but experience has taught me that favourable results obtained by such means do not always come out the same in actual practice.

Mr. Varley says it is immaterial in what part of the circuit the condenser is placed. With the system by which the cables are actually worked experience teaches that it *is* material. Suppose the condenser to be placed at the sending end; at the receiving end you would have a sensitive galvanometer with 350 tons of copper encased in gutta-percha immersed in salt water connected to one terminal, and about 2,000 tons of iron immersed in the same water connected to the other terminal. Now, I have watched for hours a galvanometer so placed, and its movements were certainly very capricious, and totally unfitted it for a signalling instrument. In experiments with an artificial line in a private room the galvanometer could not be so circumstanced, and therefore it would be immaterial where the condenser was placed.

Mr. Varley states he can find nothing to confirm my statement that in 1860 he informed the Atlantic Company that to insure eight or nine words per minute through an Atlantic Cable the conductor must weigh 1150 lbs., and the gutta-percha 1020 lbs. per knot. In Mr. Varley's evidence, given on the 5th January, 1860, before the joint committee appointed by the Lords of the Privy Council for Trade and the Atlantic Telegraph Company, he states, to insure the speed I have mentioned (eight or nine words per minute), the copper must be ¼-inch in diameter, covered with

gutta-percha ¼-inch thick. My giving the weights of these dimensions and not the measurements as given by Mr. Varley, may have misled him.

Now, gentlemen, I think I have answered, and I hope satisfactorily, all the main points raised by Mr. Varley. And in conclusion, allow me to remark that the object of my paper was not to extol anything I may myself have done, nor to detract in any way from the merits due to others. But if I understand the object of this Society, it is that its members should meet and discuss amicably for their mutual advantage subjects appertaining to a knowledge of that wonderful and mysterious power, Electricity. I candidly admit I have obtained knowledge under these circumstances, and if my endeavour to impart the same has in the slightest degree been successful, I am amply repaid for all the time and labour bestowed upon my paper.

The PRESIDENT: There has been a discussion between two eminent authorities, and they appear not entirely to concide with regard to circumstances and dates, and they also differ somewhat with regard to the inferences which they deduce. With regard to questions of fact, I must say I think Mr. Willoughby Smith has given us a very circumstantial and clear statement on his own behalf. With respect to the scientific differences I would say, who shall decide when doctors disagree? It certainly is not for me to say anything upon the matter. I think you will all agree with the sentiment expressed by Sir William Thomson in the letter which has been read, in which he says he knows Mr. Willoughby Smith to be a good man, and I am sure you will cordially join in giving your thanks to him for the interesting and valuable paper he has read, and you will also, I feel equally sure, thank Mr. Graves for his paper upon a matter cognate to that of Mr. Willoughby Smith. I regret that we have not been able to read that gentleman's supplemental communication this evening, but as already mentioned it will appear in the *Journal.*

The vote of thanks was unanimously accorded.

The PRESIDENT: I have now to ask your attention, gentlemen, to a paper by Professor Hughes, on some "Experimental Researches into the means of Preventing Induction in Lateral Wires."

EXPERIMENTAL RESEARCHES INTO MEANS OF PREVENTING INDUCTION UPON LATERAL WIRES.

BY PROF. D. E. HUGHES.

The induction upon lateral wires has of late years been a serious question upon telegraph lines—the constantly increasing number of wires upon the same poles, added to the adoption of high speed and consequent sensitive apparatus, renders the study of these effects of the first importance.

The disturbing effects have become each year more and more remarked; and in 1868, by the desire of the French Telegraph Administration, I undertook a series of practical experiments, with a view of finding a remedy. I at once perceived that we had to deal with a question of a more complicated nature than was at first supposed. We found then that we had to deal with the static charge of its own line, and the dynamic induction of the lateral wires; that the effects of each were very different; and whilst it was easy to deal with the comparative feeble static charge, the more powerful and rapid effects of dynamic induction could hardly be suppressed. By introducing extra resistance coils we could reduce the induced current, but, at the same time, we should reduce the current with which the line was working. The remedy was in most cases (particularly during faulty insulation) worse than the disease. Condensers and polarization batteries would allow the momentary induced currents to act, whilst it prevented the more prolonged currents of the distant station. Ordinary Induction coils would allow of compensation if we had only two wires to compensate for; but as we had to consider ten or more wires they were useless for that purpose. The only remedy we then found was by the introduction of an electro-magnet, with an armature fixed upon its poles—thus being a closed magnetic condenser. This practically absorbed or destroyed all the short

momentary currents; but then if on this line the apparatus was working at high speed, with consequent short currents, we practically cut off the best part of the current needed.

In this case, whilst the remedy was practically perfect for an apparatus at low speed with comparative long currents, it was useless for those working at high speed requiring shorter currents, and consequently more sensitive organs of reception.

Other duties prevented me at that date from following up this line of research. The disturbances on the lines from this cause have been on a constant increase from the adoption of more rapid, and consequently more sensitive, organs; and in the telephone we have at last arrived at an organ of rapidity and sensitiveness, which not only reveals the constant induction, but which is the main reason why the telephone has not been more largely adopted upon telegraph lines.

In a late paper upon induction* I brought to notice the powerful organs of research in this field that we possess in the telephone and microphone; and we thought that by the aid of these we might investigate, theoretically, the cause and remedy for lateral voltaic induction.

The following experiments are a *résumé* of the results obtained :—

We made use of three separate and distinct means of research. The first consisted of two or more parallel wires, 20 feet long: the one being used as a primary was connected to battery and microphone, with clock as source of sound (both ends of wire being to earth). The secondary wire, upon which the induced current was observed, was connected to telephone—both ends of this line also being to earth.

The second arrangement was made, in order to study the effects upon a much shorter length, and with more accuracy than could easily be obtained on the longer line.

Two parallel—1 millimetre, copper wires were strung upon a board, 15 × 15 inches; these wires were placed 10 inches apart, and both united at one end with 1 Daniell element placed near.

* "Comptes rendus de l'Academie des Sciences," Paris, Dec. 30th, 1878, and *Jan., 1879.*

The free ends of these wires could be put alternately in connection with battery; and thus, if we surrounded one of these wires with tin-foil, or other means of protecting wires, we could observe the degree of protection obtained, by comparing it with the unprotected wire. A difficulty was at once felt in obtaining sufficient induction upon a parallel wire of such a short length. We obviated this, however, by the construction of a flat rectangular coil of 100 turns, having 4 inches side exterior. Now, by placing either of the sides of this coil near the primary wire, we had 100 parallel insulated wires, of a few inches—the influence, therefore, was multiplied greatly; we had also in this coil, on the opposite side, 100 parallel layers running in a contrary direction, so the resultant strength of current was simply that due to the different distances; and it was in order to diminish this, as far as practicable, that we made the coil square. The sound obtained by this means was amply sufficient for all the researches needed.

The third means was that usually employed, namely, the use of coils either superposed or wound together in the same helix; by this means powerful effects could be easily obtained: and as the results were identical, and verified by the three systems, we will at once relate the experiments with coils.

I. If we construct two flat coils of 100 yards of No. 30 copper wire each, and join one as primary to battery and microphone: the second coil as secondary, joined to telephone alone, we find that by placing one of these coils near, or within a few inches distance from the other, we hear distinctly the sounds of the clock on the microphone. Our object now being to screen the secondary coil, so as to reduce the induced current, and consequently sound obtained on telephone, we joined both coils to the same earth plate, without effect. We then took a sheet of copper, 1 millimetre thick, 12 inches square, and placed it between the coils, without effect; joined both coils to this plate, as if it were an earth plate between the coils, but no perceptible reduction of the induced current was thus obtained. We then put 10 thin $\frac{1}{32}$-inch iron plates, 18 inches square, between the coils, again joining them as earth, but still no perceptible reduction.

II. The two coils were surrounded with several layers of tin

foil, thus enclosing each separately in a continuous metallic sheath. This, however, had no effect, and sounds were quite as loud, even when the sheaths were joined to earth. Placing both these metallic-covered coils in a vase of salt water, and all to earth had not the slightest effect, even when the copper plate was also interposed between the coils. Theoretically, these two coils here represented a submarine cable; and as we shall see by some following experiments, neither the sea water, nor even a metallic sheath or tube around each wire would entirely prevent dynamic induction; and this has been proved in practice, as, upon a trial of the telephone between Dover and Calais, a Morse message was clearly read by Mr. Bordeaux, simply by the induced current from another wire in the same cable.

III. A single coil was made, having four separate insulated wires all wound together in the same direction. We could thus join these coils at their free ends, making several combinations each giving very different results. We will call these separate coils Nos. 1, 2, 3, and 4—the microphone remaining on the primary, and telephone on the secondary. No. 1 joined to microphone, No. 4 to telephone—induction strong, and consequently sounds loud and clear. Nos. 1, 2, 3, to microphone, all same direction, No. 4 to telephone—no perceptible increase of sound by the increased quantity or surface: similar results when 1, 2 to microphone, and 3, 4 to telephone.

Thus by no combination of parallel wires can we hope to diminish induction; but if we join Nos. 1 and 2 to microphone, so that the current from 1 returns by 2, then we have a complete screen—no sound whatever being now heard on the secondary coil No. 4. The same effect takes place if we join 1, alone, to microphone, and 3 returning by 4 to telephone. Consequently, if a telegraph line or a telephone line had a return wire upon the same poles, and absolutely equidistant from the inducing wire, we should have perfect protection, from the fact that the primary would then induce parallel currents in both wires in the same direction, but contrary to itself and these parallel currents would, being of equal force, neutralize each other. The remedy here is *complete, although* it is true that it would introduce a double

resistance, and the cost of a double wire cable would probably prevent its use. Leaving, however, the practical objections to its use aside, we shall see by some following experiments that the remedy as regards prevention is absolute.

IV. Taking this same 4-wire coil, and joining 1, 2, and 3 to microphone, so that current goes through 1, returns by 2, and back again by 3, we then hear on No. 4 only one half the induction that would be caused by either wire alone.

V. Using still the same 4-wire coil, and sending by 1 and returning by 2, no sounds are heard as we have already shown, but if we short circuit either 1 or 2 we at once hear strongly the sounds induced by the coil that is not short circuited, in this case without interrupting the flow of the current or breaking the communications. We have produced or annulled an induced current merely by cutting off its protecting contrary direction coil, and if the microphone instead of being placed in the direct primary circuit is placed between the junction of 1 and 2, and either coil, we hear the sounds not by any variation in the primary circuit but simply by the more or less protective influence of the return wire.

VI. If instead of a return wire of the same length in this 4-wire coil we use a shorter wire in the same coil, it does not protect it, except for the same length as the protective wire; and if as return we use an exterior coil at some distance apart but of same length as the primary, it does not protect it at all; this is evident, as from theoretical considerations we know that the return wire must be equidistant, and should be of equal length and diameter.

VII. Knowing that a metallic envelope diminishes in a slight degree the induction—first, by the work done in inducing parallel currents in this wire itself when on a closed circuit, and, secondly, by the consequent diminution of force after such work—we tried how the many similar closed coils, introduced between the primary and secondary, would reduce the amount of induction to one-quarter of its original amount. The primary and secondary being placed 6 inches apart, the introduction of one or two similar coils between them gave no perceptible reduction of force, and it required twelve such coils to reduce the induction to one-quarter of the original amount, and this reduction only took place if the interposed

coils had closed circuits; the instant the circuits were opened there was no perceptible difference with or without the twelve interposed coils. From this we draw the conclusion that the only reduction takes place when work is done in interposed coils by being on a closed circuit, and that it would require a tube of metal to surround a conductor at least twelve times the amount of primary wire, in order to reduce its inductive effects to the one-quarter as above stated.

VIII. The results obtained with coils were now applied to two lateral wires in a straight line 20 feet long—the primary being insulated by gutta-percha, and covered externally with tin-foil several times. Here we had the exact conditions which have been more than once proposed as a remedy or prevention of induction for the use of telephone lines. This covering, however, was found to be no protection at all, the induction was equally as strong with exterior coating to earth or open—theoretically there should have been a slight, and, as we have already seen, *very* slight reduction, but the difference was not perceptible. The induction through this protective covering gave loud sounds on the telephone, but if we used the outer tin-foil covering instead of a return wire for primary, then no sounds at all were heard; in fact it formed a perfect protection, as we have already seen, in the 4-wire coil.

IX. The short local wire of 15 inches already spoken of as the "second arrangement," was now used. The primary wire was led through a glass tube 12 inches long, which had 10 coverings of tin-foil, all joined to earth. This arrangement proved, as above stated, of no avail; the current however being returned by the tin-foil covering proved a perfect protection. In addition to the 10 tin-foil coatings we added 20 coatings or cylinders of thin charcoal iron —making an exterior diameter of four inches, and a total thickness of metallic sheath of ¼-inch between primary and secondary wire. As but little reduction was perceptible, we placed a magnetic needle, suspended by a silk thread, at a distance of 4 inches from the primary, and measured the deflection by current. Without any protective coating it gave 3 degrees deflection, with glass tube and *10 tin-foils to* earth 3 degrees, with the additional 20 iron cylinders

about $2\frac{1}{2}$ degrees, thus the magnetic lines of the voltaic current pass easily through non-magnetic and magnetic bodies, and as the dynamo induction is due to the *change* in this field of force, it is easy to see why the numerous metallic sheaths failed to arrest the inductive force.

We have seen by the preceding experiments that we should have perfect freedom from induction if we employed a return wire at equal distances from the inductive wire, and instead of a metallic sheath as we have seen might serve, it would be best to employ two separate gutta-percha or insulated wires, and in order that each wire should have absolute equal effect, a slight twist of these two wires should be given, this twist would then successively bring each portion under equal influences.

Since making these experiments we have learnt that Mr. David Brooks, of Philadelphia, suggested a twisted double wire as above described as a preventive against induction, and that it has also been patented by Professor Bell.*

A more perfect solution of this question than that of using a double wire with one as a return, giving double resistance to that of an ordinary wire, would be to use a double wire having a slight twist, each wire acting as an independent line with its own earth at each station, and its own separate battery at the transmitting station. The transmitting instrument should be so arranged as to send simultaneously positive in one line and negative in the other—these if of equal force would exactly balance each other as regards induction on other lines. The receiving electro-magnet should be wound differentially, or, what would be still better, should have two separate wires wound together. The external end of one of these wires should be to one line, and the internal end to earth; the other wire should, on the contrary, have its internal extremity to the other line and its external extremity to earth.

The effect of this would be, that if any current of same polarity passed both wires at the same instant, no effect whatever would be felt on the balanced electro-magnet, but if two currents, each of opposite polarity, passed through the coils or electro-magnet, then

* Professor Bell has lately written to me, stating that the date of this patent is Nov. 1877.

it would work at its maximum effect; we should thus have a double line wire with less resistance than the single wire, with complete freedom from earth currents, dynamo induction, and from all extraneous disturbing causes.

If two ordinary aerial lines are thus used, they should have the twist given to these wires by changing their position relatively to other wires from vertical to horizontal, and *vice versa*, at each pole or mile—thus if we had two lines A and B they should have these four relative positions repeated as often as possible, viz.:—A B, then $\genfrac{}{}{0pt}{}{B}{A}$, then B A, and $\genfrac{}{}{0pt}{}{A}{B}$.

We have tried to find a theoretical remedy by means of which a single wire would be free from this disturbing influence, and we have found that there is no possibility of a perfect solution of this question, provided that we send and receive on alternate wires at the same end, but if all the wires send in one direction during a stated period only, or work constantly in one direction only with one set of poles, receiving by another route or set of poles, then the resolution of this question becomes not only theoretically possible, but, I believe, practically so, provided the lines are, as in France, all arranged in one plane, or nearly so—the only departure from the vertical plane consisting in the wires being alternately strung on each side of the poles—and this is perfectly followed by the compensating instrument I have the honour to present this evening for the first time to public notice.*

Suppose three or more parallel lines, A, B, and C; now, A induces a current on B and C proportional to its force of battery power and the several distances of these wires from the inducing wire. If we can send into the wires B and C an opposing current of exactly equal strength and duration to that of the induced current, we evidently reduce the induced current to zero, and have consequently no disturbing effects on those wires.

After numerous experiments, I have found that if coils of wire be so placed in relation to each other, that their relative distance

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* Mr. Chas. H. Wilson of Chicago devised a plan by means of Induction Coils permanently reversed in relation to each other. The induction could *thus be* neutralised between two wires.—"Speaking Telephone," by B. Prescott, *1878, page 362.*

represents the relative distance of the wires from each other, and if each line at its transmitting station be joined to earth through these coils, then in passing the battery current through one of these coils to its line, it would induce a current whose direction would be contrary to itself; and as this would be exactly what takes place on the line itself, we should (supposing the coils to have equal inductive force to the lines) simply double the induced current, and the disturbing effects would be double what they were before the use of the coils; but if at the same instant that we put battery to line we reverse the terminals of the coil in relation with it, then the induced current sent by the coil will be in the same direction as battery current on the primary, and this would neutralize the contrary direction of the induced current, reducing it to an absolute zero.

I have embodied this idea in the instrument which we present this evening, consisting of a series of coils of insulated iron wire. There are two of such coils, 10 centimetres diameter, 100 metres long, connected together so as to form a single line; these are placed at a distance of 10 centimetres from two similar coils, and between them, at the distance of 5 centimetres, is a single coil. We have thus two long lines, and one intermediate short one. The induction of these coils on each other represents the induction on similar iron aerial wires; the effects, however, are much greater with coils than on straight wires, and this fact allows us to reproduce, with a hundred metres of wire only, the full amount of induction on a line of a hundred miles or more. The reason of having a shorter intermediary coil is, that it represents, as we have found, the most difficult portion of the problem.

To compensate for the induction on these lines, we have three small No. 32 copper (covered) coils; these are so arranged on a graduating scale that they can be gradually approached until the exact balance for each line is found; and this is found very rapidly, for we have only to slide it until complete silence is obtained from the induced currents passing through other wires. These coils have no necessary absolute relation as to length or power to that of line, as we can increase the force by approaching them; but in the case of intermediary wires, *and also* several wires of same or unequal

length, the coils should bear a definite ratio to the length of line, say 100 metres of copper coil to 100 miles of line; but, from the powerful effects I obtain with the coils I have experimented with, I believe that 100 metres per 500 miles of line would be amply sufficient.

Each coil should be reversed by the key of the instrument at the instant of putting on the battery, and with these conditions the compensation is most perfect, and lines that were full of powerful induced currents all become perfectly free from them, whilst the method in no way militates against the transmission of the battery current on its own line. But as our object this evening is not to bring forward new instruments but to discuss the question generally, I will at once sum up my conclusions.

There are three points on ordinary telegraph line or lines where we may attempt a solution of the difficulty by compensation or other neutralising effects, namely—the receiving, intermediate, and transmitting stations.

At the *receiving station*, ordinary induction coils, or the compensating coils I have described, would be injurious, as they could not be reversed in relation to each other, and would thus increase the induction.

Voltaic currents, direct or by derivations from the primary line, would answer only where the signal consisted of a dot or a short signal exactly equal in duration and force to that of the induced current.

Polarization batteries, direct on line, would allow the induced current to pass, and if used as a derivation would require a very great resistance, and would allow the extra current from the working electro-magnet to affect all the rest.

Condensers would be similar to the polarization or secondary batteries. Resistance coils would reduce the induction, but also in equal proportion reduce the voltaic current needed. Magnetic condensers would absorb the induction, but also absorb the first part or the part most needed of the voltaic current.

At the *intermediate station* all these would give contrary effects to those desired, except secondary batteries and condensers *in derivation*; but these would weaken the primary current, and

for a line of 10 wires would, from a calculation we have made, require 45 such condensers, and even then it would be impossible to fulfil all the necessary requirements.

At the transmitting station ordinary induction coils could not be used except for a line of two wires only.

Derivations from sending battery would require that a great resistance between line and earth should be introduced, and, moreover, the current would be prolonged beyond the induced current.

Polarization batteries and condensers would also, if in derivation, require great earth resistance, and would require for a 10 wire line the same number of condensers as the intermediate station with like results.

Compensating coils, as arranged in the instrument we have submitted, fully meet the difficulty, giving perfect compensation for any number of wires, with but one coil to each wire, provided that the lines are used for transmission in one direction only for stated periods, and that they are arranged as in France, as already explained.

The perfect line, however, itself quite free from induction, and not inducing on other wires, and therefore requiring no compensating arrangement, is the double wire we have already mentioned, where both ends are to earth, and its electro-magnet wound differentially.

I have studied this subject with much interest because I believe that the future improvement in telegraphy depends in a great measure on its entire solution, and we hope that by a discussion on this subject new facts may be brought to light.

The PRESIDENT: Professor Hughes will now be good enough to make some experiments before you with his instrument.

Professor HUGHES: I regret that this instrument is not suitable for an audience like this to listen to, but I will ask the President kindly to listen to the results and describe them. I have only 3 Daniell's cells, and all the apparatus I have here is laboratory apparatus, simply made for research, and the theoretical conditions of the experiments I make are represented pictorially upon the diagram before you.

I have here 3 Daniell's *cells of* small power, a microphone, and a

little clock as the source of sound. I have here also two small coils made of 100 yards of wire, and at present I have an induced current upon it. One of these coils is in connection with the telephone and the other with the microphone.

This is simple induction. The object of this is to show you how with such small coils as these we can produce induction on 100 or even 1000 miles of line; and if we can produce with such a coil as this a powerful induction, we can then use these compensators for the induction itself; and these are only small coils of 43 ohms resistance each.

Now, it has been proposed to wind a tinfoil covering round the wire as a protection, and I believe there has been some discussion about the value of it; but it is no protection at all except relatively. You may have 20 coverings on this glass tube or this brass tube $\frac{1}{18}$th of an inch thick, but it is no protection.

If you place this [square coil] near any of these surfaces you can hear equally as well on unprotected wire as with a wire protected by a thick iron tube.

Here I have a coil of 10 iron wires protected by $\frac{1}{8}$th brass tube, but it is no protection whatever, even when joined to earth. You now hear the induction.

[The PRESIDENT: Perfectly distinct.]

And if I put it on the centre, you hear louder. That is, because when I put the coil in the centre, the current acts on both sides equally. Thus we can easily demonstrate that a metallic sheathing would be of no practical value whatever.

Here [illustrating] we have the double wire which I propose in the paper as a perfect protection against induction. On this piece we can study and hear the effect of the two. At this portion where the double wire is twisted and where the two currents pass each other we have perfect silence and consequent protection, but with this open looped portion we hear distinctly. This demonstrates the necessity of the continued twist, in order that each portion should summarily be brought at equi-distance.

Here we have an experimental instrument in which the *attempt* has been made to neutralize the induction on all adjacent *wires, so that* each shall be mutually protected, and that without

introducing an element of disturbance to the perfect working of each single line.

It is represented by a perfect induction balance by means of which all lines no matter what their comparative length resistance be, can be at once and easily balanced by a counter current of induction. In order, however, that it should be a perfect success instead of a perfect failure, the whole of the conditions as laid down in my paper must be rigidly observed.

At present the induction is not passing through the compensating coil, but if I put it here [pointing] it passes through the compensating coil, and they ought to be silent. Now it would be easy to make this silent by putting it on a broken wire which would be cheating: but if I disturb the balance you ought to hear, because in this electrical state the balance is like that of a pair of scales: if I take off the weight on one side it goes up [illustrates]. In fact a very small piece of iron will affect it. It is not iron alone which disturbs it, but any other kind of metal will do the same.

[A MEMBER:—Does wood affect it?]

No; but there are various solutions which do so. Some of my observations are in quite a different field, and the principles involved in this experimental instrument have been applied by myself in the production of a pure physical induction balance, by means of which I have studied the remarkable effects on different materials, some of which I have just roughly demonstrated. This instrument's results which have no bearing on the question before us to-night, will be shortly brought before the scientific world.

I will now put the primary on No. 2 and if you listen to No. 1 you will hear nothing. Now, when I suppress the compensating coil, you hear the real induction on that line [illustrating]. If I make numerous combinations such as working No. 1 and 3, and listening on two and in fact every conceivable combination, you will hear loud induction when the balance is not used, but perfect silence when placed and used as my paper indicates.

I have made a great many experiments with this instrument, and it is the only one that has answered all the requirements as far as the laboratory goes. *This instrument* has never been tried on

any line, but it is satisfactory to have been able to solve and vanquish all difficulties thus far.

No doubt when we try it practically there will be some difficulties; most of these I have foreseen and provided for, but an instrument that works well in the laboratory gives fair promise of success on a line. If it will not work in the laboratory, it will not work on the line. Of course it would be objectionable to have the trouble to adjust every line, and it may be a fair question if the lines at present require the use of the instrument, but I am convinced that the day is not far off when serious attention must be given to this question, and every resolution promising success will be fairly tried, and as this instrument is so extremely simple, only requiring one coil to each line, there is hope in the future of its practical value.

A vote of thanks having been (on the motion of the President) accorded to Professor Hughes, a ballot took place at which the following gentlemen were elected:—

As Foreign Members:

M. Henri Menier.	M. Jules Tessier.

Jules Hippolite Wunschendorff.

As Associates:

Joshua Frankland Bailey.	Joseph T. King.
Arthur L. Dearlove.	James Nicholson.
William Bernard Godfrey.	E. J. Perkins.

The meeting adjourned till Wednesday, the 26th of March.

The Seventy-Sixth Ordinary General Meeting of the Society was held on Wednesday Evening, March 26th, 1879, at the Institution of Civil Engineers, 25, Great George Street, Mr. W. H. Preece, Vice President, in the Chair.

The CHAIRMAN: The business of this evening will commence by the Secretary reading a paper recently sent us by Mr. Sivewright from the Cape.

SOUTH AFRICAN TELEGRAPHS.

By J. SIVEWRIGHT, M.A.

In a paper upon the Telegraph system of South Africa, I can hardly hope to bring before the Society of Telegraph Engineers any novel points of interest. The object which I rather have in view is, whilst laying before the members a cursory sketch of what is being done in the South African Colonies, to elicit from them their views upon several questions (of minor importance they may be) that have presented themselves to me during the last eighteen months.

I do not propose to enter into a history of telegraphic development in South Africa, but I pass on at once to a consideration of the system as it at present exists, and of the additions which in the course of a very few years one may fairly anticipate will be made to it. In a rough skeleton map of the Colonies, the existing wires are shown in continuous green lines, those now in course of erection in continuous blue, and those that have been recommended, but are not yet authorised, in dotted blue.

The telegraph engineer will here see at a glance that the guiding principle which has been followed in the formation of the system is that of securing alternative routes, so that in the event of an interruption occurring between any two points on the trunk lines, communication can still—in a round-about way—be maintained between them. The natural configuration of the country is

admirably adapted for this, for, looking at Pretoria and regarding it as the apex of the triangle, it will be seen that from there to Natal and the Cape there is hardly a single town of any importance which has not at least a second, and in some cases a third string to its bow in the event of the main chord of communication being cut.

The value of securing alternative routes in any properly organised telegraph system is so universally recognised that it is almost unnecessary to linger even for a moment over any consideration of it here. Still, it may not be out of place to point out that that value is largely enhanced when, in place of having a peaceful and law-abiding population to deal with, the districts traversed by the telegraph are in some cases still held by seething elements of discontent; when one native war, which was literally waged under the wire, has but very recently been brought to a successful issue, and when another, which may not improbably be carried on within sight of a second section of it, is looming ahead in the immediate future. Yet it will be seen that were the lines actually completed which are here planned out, the Griquas might rise again in revolt and cut down, as they did but a few months ago, several miles of wire, without interrupting the communication between Natal and the Cape Colony; whilst the Zulus, crossing the Disputed Territory, might make away with the whole section of line over the Drakensberg, and yet leave intact the telegraphic communication by one route between the Transvaal and Pietermaritsburg. It is true that a simultaneous rising amongst the different tribes might lead to the total isolation of any one district; but cohesion amongst the uncivilised races of South Africa in their enmity to the white man has hitherto proved itself to be next to impossible, and nothing short of a crushing disaster to any section of the Europeans would lead to a general native combination against them. If one tribe take the lead in going to war, the others stand by, or to use their own words, "sit still," until they see the result of the opening engagement, and should it be adverse to their neighbours, they continue in this condition, even although the messengers may up to the last moment have been passing to and fro between them, and every arrangement may have been thought to be complete for a *widespread uprising* amongst the disaffected clans.

A cursory examination of any telegraph system will, so far as the telegraph engineer is concerned, resolve itself into two main branches:—1. The construction and maintenance of the lines. 2. The apparatus with which the lines should be fitted, and the methods of working best adapted to the circumstances of the country.

In looking briefly at some of the points connected with the erection of telegraph lines in South Africa, I propose dealing with this branch of my subject under the three heads of—

I. Route.

II. Material.

III. Special Points of Construction.

Much, if, in fact, not all, of what I have to say will probably be found to apply with equal force to the construction of telegraphs in other colonies, and might appear more appropriately in a paper on "Colonial Telegraphs" than in one on "South African Telegraphs." If this is so, I can only hope that what follows will serve to elicit the opinions on the points that may be raised of those members of the Society of Telegraph Engineers who have a wide colonial acquaintance and a longer colonial experience than I can as yet lay claim to.

I. Route.

In selecting the route for a telegraph line in South Africa, there is happily no need for fighting the old battle between Road and Rail, which has been so fiercely contested in England. Railways are only in their infancy in these Colonies, and the telegraphs both in age and growth have far outstripped them. It happens, too, that the telegraphs which have been already constructed, serving, as they do, almost all the towns of any importance, take broadly the line of country which the railways eventually will follow. Stations that may be established in the future can thus be served by short loops or spurs from the main trunk lines. Nor is there the same field here for the exercise of that tact and knowledge of human character which the telegraph engineer has so often to call to his aid in England. The fate of these telegraph lines does not hang on the beck and nod of some petty landowner

or crotchety householder, for the idea of "consents" has not yet reached thus far, and "way-leaves" is a term the meaning of which is still all but unknown. Under these circumstances the selection of a route would appear to be an extremely easy and simple affair. But it is this very plethora of land, and freedom to go where one will, that renders the question not so easy of solution as would at first sight appear.

When the line from Cape Town to Port Elizabeth was first constructed by Messrs. Siemens Brothers some 17 to 18 years ago, the general instructions, as I have been informed, which were laid down for the guidance of the surveying party were to conceal the wire and posts as much as possible from the high road. Why this should have been so, it is somewhat difficult to imagine although it is but fair to add that an eminent measure of success has been attained in carrying them out.

Throughout its length of 500 miles this line can as a rule be followed on horseback at little more than a walking pace, whilst in many places the country is utterly impassable for a horse, and requires a man with some little experience of mountain-climbing to make his way along it.

The only possible reason that can suggest itself to my mind for taking such a route, is that it may have been thought that in this way wilful or thoughtless damage to the line could best be avoided. But a very little reflection will serve to show that this is an argument which carries simply on the surface whatever weight it may possess, and will not bear examination.

Under any circumstances there would be but very little temptation to steal the materials, the wooden poles are not inviting objects for theft, and the iron wire has not the intrinsic value which would entice the thief, while the insulators except for their own special purpose possess no value whatever.

And as regards damage from wilful mischief there would have been far more likelihood of this occurring amongst the "kloofs," *

* Kloof, allied to the word "cleft," is the term applied to a valley or mountain gorge which in most cases has resulted from the action of water.

There is no word in the English language which completely covers it; the *nearest approach* that I know of is the Lowland Scotch "kleugh."

and comparatively out-of-the-way districts which the line at present crosses, than had it broadly followed the beaten highway and been as a general rule visible from it. The evil-doer would then have run a far greater risk of being detected in his work of destruction than is at present the case. Thoughtless injury which is more likely to arise when the line borders on the highway might very soon have been effectually put a stop to by bringing an offender now and then before the resident magistrate: for a wholesome dread of the law can readily be inculcated into the minds of the native population, whether white or black, who inhabit the older established districts of the Cape Colony.

Having before them a country which is almost entirely scrub-bush, "veldt" * with hardly a single tree deserving of the name on the face of it; having also a fairly well-established road as a guide, the surveying party might have laid out a line any distance from ten up to a hundred yards off which would have gladdened the heart of the Telegraph Engineer. The comparative advantages of such a line it is almost needless to enumerate; not only would the prime cost of construction, but also that of after-maintenance be less; faults could be more readily rectified, and there would also, in many cases be at least a likelihood of gaining information of an impending break-down, either from the mail cart driver or the general traveller so as to be able to anticipate actual interruption to the working, and this, in a climate and over a soil so dry as those of South Africa are for a large period of the year, is a point of no small importance.

Again, by broadly following the road, provision might have been made against the river crossings which on the older lines of the Cape Colony have proved a fruitful source of trouble. The rivers with very few exceptions are during the dry season, if not wholly destitute of water, little more than brooks brawling over their rocky beds, and fordable at any point, but owing to the mountainous nature of the country they after the rain become rapidly transformed into swollen torrents that can only here and there be crossed. Bridges until of recent years have been almost unknown,

* "Veldt" is the general name applied to the country not yet under cultivation.

and although an occasional pont is to be met with, the "drift"* is still the general form of river-crossing in South Africa.

To these points the road will always make, and the lineman following the telegraph erected along it can readily cross and continue on his journey, whereas as at present rivers are crossed by the telegraph wires at spots which necessitate in many cases a detour of several miles being made by him before the examination of the line can be resumed. Instances are not wanting where zealous officers in attempting to swim at these points of crossing have been swept away and drowned.

After the bush-veldt is left and the grassy uplands of the Eastern Province, the long stretching plains of the Orange Free State, and the low rolling hills of the Transvaal are reached, the arguments for adhering to the roads lose much of their force. The roads are now less defined, river crossings owing to there being no rivers, or to the lines running as a general rule parallel with them where they do exist are few and far between; a bullock waggon for the distribution of the stores can make its way, it may be slowly, along any portion of the veldt, while a man on foot or cantering on horseback will often prefer striking across country to following the beaten track. The case is much the same (with the exception of the river crossings where the "drifts" should always be aimed at) in Kaffraria and Natal.

To sum up briefly what has been said the orders which now are in force, are:—

1. Over a bush or bush-veldt country the line of telegraph should broadly follow the highway, and be as a rule visible from it.

2. Over the grass-veldt the line should be carried by the shortest available route which can be traversed by a bullock waggon and followed by a man on foot or riding his horse at an easy canter. Due provisions should in all cases be made for the river crossings.

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* Drift is the name applied to the point in a river where, the bed of the stream being less confined, the water can spread over a larger surface and so admit of its being crossed; it corresponds in a great measure to our English word "ford."

II. Materials.

There is something to be said and much I believe yet to be done in the way of improvement so far as Colonial Telegraphs are concerned in each of the three classes of materials employed in the construction of an open line of Telegraph.

1. *Poles.*—On the very threshold of any enquiry into the subject of poles, the standing problem presents itself to us, whether is wood or iron to be employed? All the old oft-quoted arguments for and against each can be again brought forward. In favour of iron versus wood, it will be pointed out how all classes of timber are liable to internal decay or to ravages from white ants: what havoc bush or grass fires will play with such a line; how in a country so destitute of timber as is almost the whole of South Africa except the sea-board, the poles will to a certainty be chopped up for firewood, cut down as *"disselbooms," shed supports, or for the thousand and one other purposes to which they may be turned. There will on the other hand be urged against iron, its increased prime cost and the electrical disadvantages in damp weather, attending its employment.

The conclusion to which I have come, viewing the question purely from the standpoint of a telegraph engineer, is this:

Use timber where anything like a good sound quality can be obtained, and failing that have recourse to iron. But, looking at the matter as a colonist as well as a Telegraph Engineer, I would go even farther than this and say:

Use timber although not of the very first quality, and by a properly organised system of maintenance make provision from time to time for "spurring" it as may be found necessary at the wind and water line.

For I hold, although I can hardly expect the members of the Society of Telegraph Engineers to take the same view—that it is desirable in carrying out the public works in any country, but above all in a new country like this, to take advantage to the fullest possible extent of its natural products; and thus it seems to me to be a wise economy in the long run to keep the money in the

* The "Disselboom" is the name given to the pole of the bullock waggon.

country and send it circulating, instead of driving it forth to be replaced by such a non-producing article as an iron telegraph pole undoubtedly is.

Care must of course be taken not to carry out this principle *ad absurdum*, but no difficulty would be experienced in fixing a limit beyond which it is no longer applicable.

The experience which has been gained of wood as telegraph supports in South Africa, does not warrant an authoritative expression of opinion being given upon more than a few of the numerous species which are to be met with; many have been tried, but owing to no record having been kept and no reliable information being obtainable as to the number of years that some of them have been in the ground, a further period of probation must be allowed them. There are others, however, about which there can be no question and whose claims can be settled either one way or the other with certainty. In and around Cape Town the fir grows with a luxuriance and rapidity which I have never seen equalled, and which are certainly unknown to it in the countries of the north. It is as a consequence open grained and the sap is never really down. Tried for telegraph poles it has been found sadly wanting, and in its natural condition is altogether inapplicable. The only preservative process which has been applied to the fir here is Boucherising, and even that, so far as I can gather both in point of expense and durability, can hardly be said to have proved a success.

Creosoting, the only really effective process is out of the question for the oil is scarcely procurable; it is not yet to be had locally, and is regarded as a most objectionable cargo. In fact, for the railways, all the sleepers are imported ready creosoted from England. How far this may be altered in after years when the coal mines of South Africa are worked and the railways admit of native creosote being employed on native timber, is one of those problems on which it is idle at the present moment to speculate. A species of fir known as the silver-fir which grows freely on the southern slope of the Table Mountain and is almost confined to that locality, is said to possess lasting qualities when fixed in the ground, but the supply is very limited, and it would be little

short of vandalism to uproot one of the most pleasing features in what is at present an almost idealistic landscape.

There is no other timber in the Western Province of the Cape Colony which is at all available; the oak is out of the question and the Australian Blue Gum (*Eucalyptus Globulus*) which is being extensively introduced into all the South African Colonies, and which grows with marvellous rapidity is likewise inadmissible.

It is not until the magnificent forests of the Knysna and the Zitzikâmma are reached that a wider selection of native woods becomes possible, and where varieties are to be met with whose qualities in the ground are still to a considerable extent unknown.

Timber of slow growth, and permeated with as large a quantity as it can carry of some permanent oil, is, of course, what should be aimed at. Unfortunately, these are the qualities which are most conspicuous by their absence, for nature here seems to have run riot, and the vegetation is of the most luxuriant character. Those trees which are of slow growth become dwarfish and stunted, and never assume the proportions of a tree, but develop instead into large bushes.

Such is to a very great extent the olive tree—commonly known as the *waar olyven hout*—whose durability is second to that of—I had almost said none, but certainly of very few woods with which I am acquainted. Were this anything like general, and procurable of sufficient size for telegraph poles, it would set at rest for ever the iron *versus* wood question in favour of the latter. I have examined some two to three hundred specimens of this wood which had been in the ground for a period varying from 12 to 18 years, but in no case could I trace the slightest symptom of decay. The portion above ground, which was wood of a different sort, had become warped and split, or the bolts used in the scarfing had drawn, but the olive at the wind and water line remained as sound as the day on which it was first planted. The timbers which are now being most extensively used from these forests are a species of "Iron" wood (purely a local designation, and not to be confounded with the woods in other parts of the world bearing the same name), known as Black Iron wood in the Knysna, and Hard Pear in the Zitzikâmma. The poles are sawn from old trees, and are 21 feet

or more in length, tapering from 7 to 8 inches square at the bottom to 3½ or 4 inches at the top, according to the number of wires they are expected to carry. They are pointed at the top, and roofs are not employed.

Passing to the Eastern Province, we here meet with the first really sound timber which is procurable in sufficient quantities, and which grows to a sufficient height to render it available for telegraph poles. It is known among the Kaffirs as "umtâtie," and amongst the European settlers as "sneezewood," from the effect resembling that of snuff which the sawdust from it has upon the nostrils. It is a timber of extremely slow growth, and possesses a very large amount of permanent oil. Trees of natural growth 21 feet in length, tapering from 20 inches in circumference round the butt to 10 inches round the top, must be at the very least 60 years of age, and some whose opinions are entitled to weight have gone so far as to assert that a large number of the poles of these dimensions are from trees not less than one hundred years old. The primitive Kaffir has no idea of the measurement of time, and, consequently, his opinion on the point is of little value.

The permanent oil is a gum resin readily soluble in alcohol or methylated spirits, and thrown down as a white milkly deposit after the addition of water. The poles are crooked, and scarcely one is to be found straight, so that to an eye accustomed to the graceful larch or fir poles of Europe, a sneezewood line presents a very makeshift appearance. The unsightliness of the poles is, however, more than counterbalanced by their inherent excellence. A sneezewood tree may be regarded as composed of three parts: the bark, the sap wood, and the heart wood. The bark is extremely fine, and is stripped from the poles when cut. The sap wood, which in the telegraph poles is about half an inch in thickness, after a few years loses the sap, and may be easily removed. The wood itself, so far as I can learn or judge, is practically indestructible. I have examined some two to three thousand poles that have been in the ground for periods ranging from ten to sixteen years, but in no single case have I succeeded in detecting even the symptoms of decay. A piece cut from any of these poles will still burn as *brightly and freely* as good fresh resinous or creosoted timber. An

interesting feature to be noted in connection with this wood is the occasional growth of a fungus—named by the Kaffirs "vittie"—which, whilst the tree is growing, causes a peculiar liver-shaped swelling. This fungus resembles touchwood, and burns exactly like it; curiously enough, it dies with the tree. Poles affected with vittie are now rejected; but on some of those which had been passed on the earlier lines the previous existence of the fungus can be traced by the woodpecker, who, having found the only vulnerable point, has built his nest there. The retention of the gum resin, and consequent ease with which the wood may be set ablaze, render the poles liable to be tampered with when the camp fire has to be lit. For this reason, and, in fact, to protect the line from thoughtless injury, "outspans" * should be avoided, and a slight detour is, as a rule, made to keep clear of them. But the crooked nature of the poles renders them anything but an inviting object for the transport driver, even when hard pressed for a disselboom to his waggon. In fact, the objection to the employment of timber for this reason, which is so often quoted in South Africa, is purely imaginary, and I have searched in vain for a single authenticated case in support of it. As regards bush or grass fires, danger from them can, as a rule, be effectually guarded against by clearing a space, say 12 feet in diameter around each pole when the line is first erected, and maintaining it so afterwards. I have heard of large sections of wooden lines, where this precaution has been omitted in the bush veldt country, being destroyed by fire, but I know of no instance where standing poles, whether of sneezewood or any other wood, have been destroyed, although occasionally they may be well charred by a grass fire. The fire passes along so quickly that before the heat has time to accumulate the danger is gone. The average weight of a sneezewood pole is 225 pounds; and for lines far inland light iron poles possess a decided advantage in this respect even over so excellent a quality of timber as this. It is, however, by no means an easy matter to draw the line, and say at what distance from a forest, where suitable timber is to be got, it

* "Outspans" are recognised or authorised halting stages, where the bullocks are turned out to graze on the veldt, and where, in consequence, the cooking is done, and the *camp is pitched.*

would be desirable to abandon the latter in favour of the former. All would depend upon the rates of transport, which vary with the season.

When the line to the Diamond Fields was erected, now about three years ago, the average cost to the contractor of each iron pole as it lay at its peg between Colesberg and Kimberley could not have been less than 50*l*. At the present time sneezewood poles of the first quality are being supplied on the Fauresmith Bloemfontein section, an average distance of 250 miles from the base of supply in the forests of the Amatola Mountains, for 40*l* each at the peg. The rates for transport, too, which have ruled during the present season are, owing to the drought and a variety of causes, higher than any that have prevailed in South Africa within the recollection of the oldest settler. Lightness combined with the requisite strength is the point to be aimed at in the supply of iron poles for Colonial telegraphs; and in place of working on the base, which in several cases has been sacrificed to secure a diminution of weight, it might be worth while to try a combination of the ribbed base, or buckle plate with a ribbon style of tube, fitted to carry two, or at most three, wires. On every wooden line in South Africa where sneezewood is not employed, I propose that eventually it shall be used at the wind and water line. Sawn spurs four feet six inches in length and three inches in thickness, secured by three bolts, will be made use of for the purpose, and this the ordinary maintenance staff, aided by two or three native labourers, can from time to time, as it becomes necessary, see to. In a purely grazing country, such as a very large portion of South Africa is and always must be, there is one advantage which iron possesses over wood to which I do not recollect ever having seen any reference made. The cattle are far less likely to make a rubbing post out of an iron pole than they are out of a wooden one. The former gets so readily heated in hot weather and cooled in cold weather, that the bullock, having once laid his neck against it, gives it a wide berth in future, whereas many of the wooden poles are worn almost as smooth as a glove from this cause. The advantage becomes, of course, greater when two or more wires, liable to be swung into contact, are carried along the same line of poles.

II. Wire.

The wire in general use is No. 6, and except in occasional cases of railway lines or purely local circuits, where No. 8 might be employed, no smaller gauge should be adopted. It must not be forgotten that as each new section is pushed on it is not a separate line of fifty, eighty, or a hundred miles which has to be erected but it is an additional link that has to be added to a chain already several hundred miles in length. Thus the Bloemfontein-Fauresmith line now in course of erection although apparently an independent line is really but an extension of the Fauresmith-Fort Beaufort, Fauresmith-Port Elizabeth, or Fauresmith-Cape Town circuit, and bearing this in mind it will be at once seen why no smaller gauge than No. 6 galvanized iron wire should be employed. It is worthy of remark that an ungalvanized wire erected by Messrs. Siemens Brothers towards the end of 1873 upon the line between Fort Beaufort and Colesberg has stood remarkably well. There is no better working wire in South Africa than this. It has not been broken since May, 1877, at least, nor does it up to the present time show any signs of corrosion. Previous to erection the wire I believe underwent some process of oiling by the manufacturers.

There can be little question that the greatest desideratum in the materials now employed in colonial telegraphs is a lighter form of wire which will give the same resistance as the larger gauge of iron wire now in general use. The undoubted improvements which have of recent years been introduced into the manufacture of the latter giving a No. 7 wire with the resistance of the old No. 4 go a little way, yet only a little way towards meeting the difficulty. But if some form of compound wire could only be guaranteed of equal durability with the iron, and give at the same time with a diminished gauge a lower resistance there is no boon which would be more gladly hailed by the colonial telegraph engineer; for the transport factor which for many years to come must be the standing difficulty in all the colonies, varies, one might also say directly as the gauge of the wire. The wire is the central point in the materials employed in telegraph construction, and a lighter wire means lighter poles, lighter insulators and a general diminution in weight

of what for want of a better word may be called the "fittings" of a line of telegraph.

It is needless to urge the importance of this for every colonial telegraph engineer would bear his testimony to it, but it so happens that at the present moment the force of what I have said is brought home to me in a practical manner which is decidedly the reverse of pleasant. In the construction of the line to the northern border of Natal, and from Pietermaritzburg to the Transvaal, transport from a variety of causes is procurable at nothing short of ruinous rates. Suffice it to state that £15 10s. per ton is the rate which rules as I write (December 9th) between Durban and Maritzburg a distance of 54 miles, whilst an occasional waggon might as a favour be picked up to take a load up country to Pretoria, 450 miles off, but at nothing less than £52 10s. per ton.

III. Insulators.

The question as to the best form of insulator for South Africa has recently been under discussion and Mr. W. H. Preece in his capacity of Consulting Engineer to the Crown Agents for the Colonies has written an exhaustive report on the subject. On coming to the Cape Colony this was one of the points to which my attention as Special Commissioner was drawn, and I was asked to report on the relative merits of three classes:—The iron hooded, the Andrews' porcelain, and Oppenheimer's Australian pattern. It has been impossible to make any comparative test as yet of their electrical properties in this climate, but of course what holds in other countries will hold also here, and I know of no better trial ground for the electrical qualities of an insulator than the neighbourhood of London during November. We have our dry seasons, and our wet seasons, and mists and fogs are not unknown in South Africa so that the electrical power of an insulator must here, as everywhere else, be a high point in its favour. In this respect Andrews' and Oppenheimer's are of course decidedly superior to the iron-hooded, and consequently where wilful damage from unsettled natives, or accidental damage from falling trees and violent hailstorms is not to be feared, they are to be preferred. I have recently seen for the first time in South Africa the ordinary

double shed brown earthenware insulators which were put up a few months ago on a line skirting the railway now in course of construction from Durban to Maritzburg. They are a most decided failure. Their insulating qualities are good enough, (although all the poles are iron) except during the thick fogs which frequently prevail through the summer months on the Inchanga over which the line passes, and at such times the line exhibits the same symptoms of weakness as the English lines similarly insulated occasionally do during the autumn mists; the insulators are *mechanically* equal to their work, but they are utterly powerless against the lightning, and never does a thunderstorm, approaching any degree of violence pass over the district without smashing to pieces three, four, five, or sometimes as many as eight or ten of them. I have examined the binders and fragments of several of the insulators thus smashed and the cause of the damage is evident enough:—

A heavy charge enters the wire and at each insulator may be supposed to pass round the binder as well; the wire and binders become in fact the inner coating of a Leyden jar whose outer coating consists of the bolts of the insulators, and they on an iron line are in direct metallic contact with the earth: the section of the brown earthenware which plays the part of a dielectric fails to prevent disruptive discharge from taking place, and the result is the destruction of the insulator and the wire brought direct to earth. At the point where the discharge takes place (and in those which I examined that point was not where the line wire touches the insulator) a small globule seemed to have been raised on the wire owing no doubt to the heat evolved there at the moment of discharge whilst a black discolouration was plainly visible on the brown earthenware. It seemed as though the mechanical structure of the insulator provided a passage of minimum resistance at that point. How far this grave objection to the employment of brown earthenware insulators, in a country so rife with violent thunderstorms as South Africa is, would be overcome by the employment of wooden in place of iron poles, I am not prepared to say; but that they are more liable to damage from lightning than the iron hoods is proved by the fact that on this same line and over the

section where the damage has been greatest, a wire insulated with Siemens' iron hoods is also carried; only one of these has been smashed since their erection 5 months ago whereas at least 50 per cent. of the brown earthenware have been destroyed during the last 18 months. The wire in the iron hoods too is a No. 8 whilst that in the brown earthenware is a No. 6, both are attached to the pole by iron brackets, that carrying the iron hood being uppermost and fully 9 inches above that holding the brown earthenware. Every pole has a lightning protector. But the smashing of even the iron-hooded insulators is not confined to iron lines; on some of the wooden lines interruptions from the same cause are by no means uncommon. Where the line crosses the Stormberg range of mountains between Dordrecht and James Town a summer has not passed since its erection without several iron-hooded insulators being from time to time destroyed. The damage as a rule takes place within a section of about 5 miles in length, and yet every pole is fitted with an earth wire of No. 6 gauge carried to its base.

The short experience of the Andrews' and Oppenheimer's does not yet warrant the expression of an opinion as to their behaviour under similar circumstances, but the point is one to which the insulator manufacturer should certainly devote his attention, when entering into competition for the supply to South Africa and countries similarly situated to it in regard to lightning.

The Andrews' Insulator seems as an insulator to leave little to be desired; if one might suggest an improvement in its form it would be to cut short the porcelain covering of the stalk a little way above where it ends at present. As they are now made, the porcelain is continued too far down, and in the event of an insulator canting at all under a severe strain the porcelain gets jammed on to the shoulder of the bolt and is probably broken. The special feature of the Oppenheimer Insulator is the absence of cement in fixing the bolt or pin into the cup, and the substitution of a screw instead. This admits of the iron and porcelain materials being packed separately—a point of no small importance where the carriage is of the roughest nature possible—and it at the same time provides for a ready renewal of the porcelain in the event of *its being* damaged. This difficulty of renewal apart from the

question of weight is a great drawback to the hook form of iron-hooded insulator, when employed on wooden and especially on sneezewood poles. When such an insulator has to be replaced it is next to impossible to remove the old hood, for the screws fixing it to the post hold so tenaciously that the top of the pole would have to be splintered before they could be removed. The new insulator has therefore to be placed beneath the old one, and the pole in consequence loses several inches of its length. With the invert form again—whether it is the Indian form of iron hood or either of the porcelain forms already named,—the iron bracket once fixed for its reception remains there as long as the pole itself stands. But there is another point in favour of Mr. Oppenheimer's pattern, on which I have come to lay great stress, and that is the facility which it offers for using wooden pins. Suitable timber for these could doubtless be obtained in any country, nor after a short experience need one anticipate any difficulty in their being manufactured locally. The substitution of a wooden for an iron pin, whether the insulator is employed on an iron or a wooden pole, cannot be regarded as other than advantageous in the case of lightning, for it stands to reason that disruptive discharge will be less likely to take place (reverting for a moment to the analogy of the Leyden jar), between a metallic inner coating and a partially wooden and partially metallic one, than would be the case between two entirely metallic coatings. But again the employment of a wooden pin brings another decided advantage with it in countries liable to such extremes of temperature during the twenty-four hours as many parts of South Africa are. The co-efficients of expansion of iron and porcelain are widely different, and consequently wherever the two are associated an unequal action must be going on. The iron expands and contracts more rapidly than the porcelain does; the latter in the case of the iron hoods seems to be broken in the process of *contraction;* the porcelain of the ordinary invert is broken by the *expansion* of the bolts. I have seen on the old line from Port Elizabeth to Graham's Town a large number of porcelains dropped from the hoods on to the wire and cut almost as clean as though a diamond had been employed right round the inside of the hood. It may be urged that this

must have taken place in the days previous to the introduction of the oxide of iron into the cement, and that it resulted not from the compression of the external covering but from the local action going on in the cement between the bolt and the porcelain. It may have been so, but only by this unequal expansion can I account for the large number of cracked porcelains—too large a number to be explained away even by colonial carelessness—in the newest pattern of iron-hooded insulators. Porcelain tried upon this same line, only the remains of which can here and there be traced, had, I am told, to be abandoned owing to the ease with which the porcelain broke from no apparent cause. I am not aware whether an india-rubber washer has ever been tried between the pin and the porcelain, but I see no reason why it should not be adopted. It would obviate the danger arising from this variation in the co-efficients of expansion, and would certainly be no drawback to the safety of the insulator in a thunderstorm.

Generally, I am inclined to give even already as my opinion, that the iron hood form of insulator should be had recourse to, only where wilful or accidental damage from terrestrial causes may be anticipated; and I would prefer for the reason given afterwards, the Indian pattern where the wire is bound into each insulator, to the hook form with the strainer every 7th pole. All other cases can I think be met by Oppenheimer's pattern, the wooden pin being for the reasons already stated preferred to the iron one.

IV.—General Points of Construction.

There are very few, if in fact any, points in the actual erection of a telegraph line which can I suppose be considered special to South Africa, but still there are several difficulties here which the telegraph constructor in England is seldom if ever called upon to face. First and foremost amongst these I place the *Road Crossings*, which are the *bête noir* of every telegraph line in the country. The roads are in many instances merely bullock waggon tracks across the veldt, and as soon as one gets cut up or at all heavy another is formed alongside of it. In this way the width of a road goes on increasing until in many instances it grows to a hundred

yards or more. The rule which holds as to bringing the last pole on each side up as close to the track as is consistent with safety now loses its significance, for a pole which is the last on one side to-day may occupy the same position on the other side to-morrow. Trenches are dug for a radius of six feet around each pole which is considered at all dangerous, and stones where procurable are in addition piled around its base. But for all that, cases of waggons running against the poles, especially during the night when much of the transport riding is done, are of anything but of rare occurrence. In dealing with this danger, which is one by no means lightly to be got over, the advocates of wood *versus* iron may claim another advantage for wooden posts. An iron tube runs a far greater risk of being smashed than a sneezewood post; the waggon in fact as a rule comes off second best in the latter case, and the transport riders to whom the road is known treat the wooden poles at the crossings as they pass with far greater respect than they do the iron when similarly employed. Since the erection of the Transkei line in the spring (September and October) of last year, there have been no fewer than seven interruptions due to the breaking of iron poles by waggons going against them between Komgha and Butterworth, a distance of 26 miles, whereas there has been no stoppage from the same cause on the rest of the line, which from Butterworth to Pietermaritzburg is poled with sneezewood. These iron poles are a light form with open bases, manufactured by the late firm of Messrs. Warden, Muirhead, & Co. It is not only the danger to the poles which has to be guarded against at the road crossings, but when two or more wires are run upon the same line the trouble which is experienced from contacts is almost incredible. These contacts are caused by the "vorslacht"* of the long whips employed by the drivers, crossings as a rule having been made so as to avoid bends in the road are usually found at some rising ground where the lash has to be most vigorously applied, and there is scarcely a single crossing from which one or

* The "Vorslacht" is the thong or lash at the end of the whip. When a span of from 16 to 20 bullocks, yoked two abreast have to be kept under way, the whip required to urge them on must necessarily be of a length that would astonish the eye of an *English driver.*

more fragments of these do not dangle in evidence of the whip having come to grief.

Poles have been placed as close as they could be with safety; straining insulators have been used and the wires stretched as tight as possible, but all to no avail. The only effectual plan which could be thought of was to run each wire some thirty or forty yards apart on a separate line of poles and let them converge at a point beyond the reach of the worslacht. This at best was but a sorry makeshift and had there been more than two wires to run, the climax of absurdity would soon have been reached. The method which I have now caused to be adopted is to employ double poles at all such road crossings, to fit these with 6 feet arms carrying one wire at each end, to shackle off on both sides, or double bind and solder the line wire on each side to an invert, and to employ light No. 12 steel wire for the actual crossing where there is danger of contacts.

I prefer using a shackle even though so detective an insulator or securing the wire firmly to an invert to employing a *straining insulator* at such points. In fact my objections to using straining insulators at all, grow with my experience of them. The principle at first seems an excellent one, and electrically the insulators are superior to shackles, at least to the present form of them. But mechanically my experience of the straining insulator would lead me to pronounce it little short of a failure. I know that it has been, and continues to be, very extensively employed in almost every quarter of the globe so that it is probable the conditions in South Africa under which it is employed vary from those which are to be met with elsewhere. There are two forms of straining insulators familiar to every telegraph engineer, the old "wedge" form and the later "eccentric" form. Without a single exception every one concerned in the construction and maintenance of telegraph lines in South Africa is in favour of the former in preference to the latter; the drawback to the employment of the wedge form is that unless great care is exercised in driving the wedge home, the line wire is apt to get "nicked," and away it goes at that point with the first sharp frost. Where long spans too have to be taken either at river crossings, or where the wire swings across extensive kloofs

there is a danger of its being worn through by friction against the wedges: for this reason I have had the ordinary hook insulators fixed at all such crossings and the strainers removed to the pole before the last on each side. It is, however, against the *eccentric* that most of the objections are levelled, and not without reason. Except on a flat or something approaching to it, where the wire lies in an almost horizontal position, the eccentric is not to be depended upon; if the ground is broken, as the greater portion of that traversed by the wire in South Africa is, the nut refuses to take a proper grip, and the wire slips under very little pressure; the force to which the wire is subject owing to the extremes of temperature is more than sufficient to draw it from the eccentric and on lines of more than two years of age which pass over an uneven tract of country, I have not seen *one* eccentric in *ten* properly standing to its work. Scarcely a week passed during the last winter that one or more of the eccentric nuts were not sent in in a broken condition; the fracture apparently having been due to the contraction of the wire during the intense cold of the night on some of the up country lines, especially those between Worcester and Beaufort West. It may be contended, and not without some show of reason, that much of this is due to the workmen not exercising sufficient care in dealing with the strainer; yet it must be remembered at the same time that the colonial telegraph engineer has few skilled workmen to choose from: he must work with the weapons that are ready to his hand, and simplicity will in consequence, be for many years to come, a very strong recommendation for any item of telegraph plant intended for colonial use.

An improvement might be made in the bracket of the straining insulator by rounding off the ridge in place of leaving it so sharply pointed as at present. The edges cut through the tackle ropes, rendering it essential that the straining up should be done by some one up the pole. Were the ridge of the bracket rounded, the straining up might more easily be done from the ground by means of the block and tackle.

In fixing the iron-hooded insulators or the iron brackets to the poles there is a singular unanimity of opinion in favour of employing the black iron coach screws in preference to those of galvanized

iron. The latter break off in very large numbers owing doubtless to the brittle quality which they acquire in the process of galvanizing, and I am inclined to think that not a few of the broken eccentrics are due to the same cause.

Provision against the effects of Lightning is another point in the construction of a line of telegraph which has to be kept more prominently in view in South Africa, than in England. Mention has been already made of the havoc which it plays amongst the brown earthenware insulators, and also on the iron hoods where the mountain ranges are crossed. But barring these the lines enjoy a singular immunity from damage especially when the number and extraordinary violence of the thunderstorms are borne in mind.

To one who has not witnessed a tropical or sub-tropical thunderstorm, not even a faint idea can be conveyed of its awful grandeur. Faraday to whom we are told all the splendours of a palace were as nothing compared with a thunderstorm on Brighton Downs, would have leaped for joy had he been a spectator of the marvellous displays of lightning which night after night are to be seen in the Eastern Province of the Cape, Natal, and the Transvaal, during the summer season. The standing wonder to me is that so little injury is done by them, and I can only account for it by the fact that the lines are comparatively so few, and that through these districts, except in occasional short lengths, they carry only one, or at the most two wires. Thus the area over which the storms play is so large that the probability of any section of the line being struck is very small.

Every pole is, of course, fitted with a lightning protector—which, as a rule, is of the same gauge as the line wire—sharpened at the point, and carried to a height of a couple of inches above the top of the pole. I was not a little surprised to find on reaching the Cape Colony that shortly before my arrival a fiat had gone forth that all lightning protectors were to be done away with, on the ground that they gave more trouble than they were worth. It appears that a notion prevailed that a lightning protector, in order to be effective, ought to be continued beyond the pole to a distance of 12 or 18 inches; only in this way, so I was gravely informed, *could* protection to the side insulators, as well as the pole, be

secured. These projecting wires, which in some cases I have seen continued to a distance of two feet or more, were from time to time driven against the line wires by the large birds perching on the poles, as well as from other causes, not the least common being insecure stapling, and full earth resulted. Although the destruction of the lightning protectors had been on the whole very ably carried out, it is needless to say that one of the first steps which I took was to have them all replaced. The difficulty with the large birds was readily met by cutting the wires to their proper length, and allowing them to project only an inch, or at most two inches, above the pole; the dangers from insecure stapling, especially upon the sneezewood lines, could not be so easily overcome; owing to the sap wood falling off in the course of a few years, the staples holding the earth wire to the pole, spring, especially if they have not been carefully driven home at first. The heart wood is so extremely hard that the ordinary No. 8 staples of the common form will scarcely venture into it, and it is always found advisable to bring the two ends closer together than is usual to admit of the staple getting a better grip. Various devices have been adopted to secure the lightning conductors. On some lines the wire is wound spirally around the pole, which would answer well enough were there no likelihood of a second wire being carried along them. If more than one wire, however, has to be erected, the chances are that the second bracket is nailed on to the lightning protector. A very good plan is to bore a hole through the top of the pole about an inch below the extreme point, and thread the lightning protector through it. This has been found so far to answer well, and where it has not been adopted, the now standing rule is carried out, to drive a couple of staples well into the heart wood at the top of the pole, and place the others six inches apart for the next three feet down, and then twelve inches apart for six feet above the ground line. This secures them from being brought against the line wire at one end, as well as from being tampered with by cattle rubbing against the poles, or by the evil disposed and thoughtless at the other.

Jointing is another point on which I was at once compelled to lay the greatest possible stress, for a soldered joint was a *rara avis*,

and seldom, if ever, was solder applied when a fracture of the line wire was made good. I should hardly care to say how many thousands of dry or imperfectly soldered joints have been cut from the South African telegraph lines during the past 18 months, and some of them certainly merited a place of honour in the cabinet of a telegraph *curio* collector. I have found the Fletcher's soldering apparatus, with an 18 to 24 inch handle, of the greatest possible aid to the maintenance staff in jointing; cotton waste steeped in paraffin, a small bottle of which the lineman invariably carries with him, is found to be, taken all in all, the best fuel for it. The antipathy to dragging about the soldering iron and fire pot prevails as largely here as at home; the latter, however, is not so frequently required, for during a large portion of the year sufficient caloric to heat the soldering iron can be obtained from the combustion of dried cow-dung, which is the common, and in fact, the only procurable fuel on the treeless grassy tracts of South Africa.

The *leading in wires* form another subject on which much might be said. The extremes of temperature to which they are subjected render it essential that they should be carefully protected. The ordinary gutta-percha, at least of the quality exported hither, is altogether unsuited, and after a very few months is worse than useless. Various forms of covered wire are now being tried, but I question much if any of them will be found to surpass the pattern of Hooper's compound employed by the Telegraph Department of India.

Earths are the last point in connection with the construction of a telegraph line on which I desire to say a few words. Owing to the absence of water, great difficulty is experienced in procuring, not a good earth, but in many places an earth at all. Up in the Karoo, where years sometimes go by without bringing a drop of rain, wires are carried to the river beds, or rather to the sandy gullies, which at times become water beds, and are attached to earth plates buried in them. But were it not for the rails, one would almost be driven back to a return wire. As it is, we are pelled to have recourse to Steinheil's historical mode of employ- the rails as a second wire, taking care that the earth wire is y secured to a chair beneath each rail, so as to avoid as far as

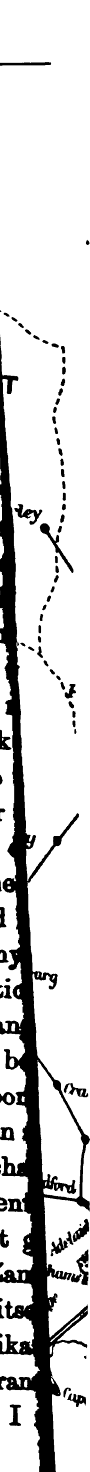
possible partial interruptions from the platelayers renewing or relaying the metals. At other stations where natural moist earth is not procurable, a pipe is carried to the earth plate and kept clear. The clerk in charge has every morning to see to as much water as this pipe can take being poured down it. It is at best but a makeshift, but it is the only feasible mode of which I have yet been able to think of obtaining earth.

The length to which my paper has already drawn itself renders it impossible for me to enter, as I had at the outset contemplated doing, into the question of the instruments and batteries employed in colonial telegraphs, or of the methods of working which it seems to me are best adapted for such a telegraph system as that of South Africa. On some future occasion I may have an opportunity of laying before the Society of Telegraph Engineers my views upon these subjects, which are fraught with interest to at least such of its members as are situated like myself. But a paper on "South African Telegraphs," even if it bore with it nothing more relating to South Africa than its name, would at the present moment be incomplete without some reference being made to that magnificent project which, I am glad to see, is now attracting the serious attention of scientific men and capitalists in England, as well as in South Africa—I mean the project of carrying a line of telegraph across the African Continent. As far back as last March I stated in a paper of considerable length, read before the South African Philosophical Society, what my views upon the question of an overland telegraph were. It is needless, even if space and time were at my disposal, to recapitulate them at length here. Suffice it to say, that after a further experience of nine months of South Africa, after travelling on foot or on horseback from King William's Town to Pretoria and discussing the project with almost every man in the country whose experience would give his opinions any weight, I see no reason for altering in one jot or one tittle, but, on the contrary, I see every reason for feeling confirmed in the views which I then expressed. Every day that passes renders the solution of the problem easier, for every day the country which would be traversed is becoming better known. I do not argue, as some would do, in favour of carrying a line almost through the

heart of the Continent, and taking advantage of the inl making it partly submarine and partly overland. The r down in the paper referred to, and which was first sugges by Mr. Merriman of Cape Town, is broadly that to whi adhere.

By forming depots at Pretoria or Kimberley, Tete Zambesi, Zanzibar, and Gondokoro the difficulties in the w undertaking reduce themselves to such as one may fairly in the erection of a line of telegraph 4000 miles in length an uncivilized or at best but a partially civilized country.

For the first 1600 miles, that is as far up as Zanzibar, far as I can see small ground for anxiety as to the succe undertaking; the Bamangwâtos whose territory is first cro eminently well disposed towards the English settlers and their chief, who is far in advance of the average native m has already asked to be taken under British rule. The Amandèbēlēs under their crafty old king Lobengūlū would careful handling, but the blow which is now being struck power of the Zulus who have always been looked up to warrior race of South Africa will make itself felt as far Zambesi, and smooth the way for many years to come in with the native tribes in that quarter. Dr. Stewart of the dale and Livingstonia missions, to whom the country drained Shirwa and around Lake Nyassa is better known than to any man living, assures me that although work will unquestio have to be done in pushing the line through, he sees no dan difficulties which by tact and careful management cannot b mounted at once. Beyond that, Dr. Kirk of Zanzibar repor line to be quite feasible with no difficulties so far as he can stop a constructing party. The weakest link in the ch undoubtedly from Zanzibar to Gondokoro, but if it is even found to be for some years impracticable the line might g along the coast to opposite Aden or, striking in from Zan along the well beaten caravan track to Ujiji, avail its need be, of the water-way afforded by Lakes Tanganyika Victoria Nyanza to the Soudan. The difficulties of tran and native management cannot be discussed here, but I



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be allowed to say a word about another objection which since the question was discussed by the Philosophical Society in Cape Town has been raised and urged principally by those in Europe who take an interest in the scheme. The general unhealthiness, especially the presence of fevers, malaria, &c., in many of the districts traversed by the wire, is quoted as an argument against carrying a line of telegraph through them. Probably a quarter of a century ago the argument would have been urged against the notion of settling Europeans in some districts, portions of the Transvaal for instance, which are now as healthy as any part of the globe, and were there only an easy means of communication inland would soon become a resort for invalids. At Bamangwâto and Gubuluwâyo white traders live and flourish. Tete is a well established Portuguese settlement whilst Livingstonia and Blantyre have already acquired a fame which speaks well for them. It is true that in the basin of the Zambesi fevers and malaria, at certain seasons of the year, prevail, but care would be taken that the constructing party entered there only at the healthy or least unhealthy time, while in the maintenance very little danger need I think be anticipated on this score. A white man would not be settled down at every pole on the route; one every two hundred miles for the work of inspection would probably be enough, and supposing this to be impossible, a very remote contingency to say the least, the "salted," and acclimatized half-caste would be found equal to the simple duties of line inspection, while for ordinary repairs, clearing the undergrowth, ramming poles and the like, the native African judging from the experience of the tribes already within reach of the wire will suffice. So long as the Kaffir can be kept from the canteen and is well looked after, and judiciously handled by his superiors so long will he make an excellent line runner. Immediately he takes to drink all his primitive good qualities for work of this description (and they are not few) speedily disappear. But enough has been said (although the subject when once mooted has a strangely enticing interest which almost chains one to it) to show that the scheme of an overland telegraph through Africa is no wild visionary dream, this is a practical proposal which to the civilizing Anglo-Saxon race wears a look of grim earnestness about

it, and I know of no undertaking which could appeal for support more strongly to all classes of that race from the philanthropist downwards, than this, for, setting commercial considerations aside altogether, I can think of none that would strike a surer blow at that "inhumanity of man to man" which at this moment in the "weird" continent more than in any other quarter of the globe, is still making "countless thousands mourn."

The CHAIRMAN: Before inviting discussion on the paper I have to propose a vote of thanks to Mr. Sivewright for his very excellent paper, and I need scarcely ask you to vote that, because there will be no dissentient. I will, therefore, ask the Secretary to convey to Mr. Sivewright our best thanks for the paper he has sent to us. We have here two or three gentlemen who have had some experience in Africa, and I will first of all ask them to favour us with the results of their experience. I see present Mr. Mayes who has not returned from Africa very long, and he had some experience I believe in the erection of lines in one part of the country mentioned by Mr. Sivewright.

Mr. W. MAYES: With regard to Mr Sivewright's paper on South African Telegraphs I concur with every word in the whole of that paper, but when he comes to speak of carrying a line through Central Africa I must differ from him. Mr. Sivewright has travelled through South Africa, but not through Central Africa, but no mere traveller, not even such a one as Colonel Grant, who travelled with Speke, can form an adequate notion of the undergrowth in that country, which is the greatest enemy to a telegraph land line being carried through it. A letter of mine in last Saturday's *Electrician* will give a pretty good idea in answer to Colonel Grant's letter in *The Daily Telegraph.* With regard to Central Africa, that is not only my experience, but that of every missionary in that part of the country. The Rev. Horace Wallis who travelled for some time with Dr. Livingstone must be a good authority not only on the subject of the undergrowth, but also with regard to the prevalence of malaria throughout large portions of that region. Dr. Kirk travelled that way, and I am quite sure he would not say that a telegraph line *could be* carried through that country. I had a long conversation

with him before I came away. Even if a line were practicable from Tete to the north of Nyassa I should like to know how you are to come along the coast of Zanzibar. The coast is one mass of tangled jungles for hundreds of miles: that is the characteristic of the whole coast, and I say it is physically impossible to carry a telegraph line through it.

With regard to the undergrowth, look at the roads I have been trying to make. In July, 1877, I constructed a road for cart and waggon traffic from the coast in a south-west direction towards the north end of Lake Nyassa, and by the end of January, 1878, it had been completed for a distance of 28 miles, but by this time the road at the other end was quite lost from overgrowth of shrubs and rank vegetation, in spite of its having been made 20 feet wide, every root as far as possible having been grubbed up, and the soil turned up at least two feet in depth. After I had constructed part of the road I was called away to make a survey on another part of the coast, from Kilwa for upwards of 150 miles into the interior to the village of Nasora, on the route taken by Baron von der Decken in 1860, and when I got back I could not find the road, shrubs from 15 to 20 feet in height having completely covered the road which had been constructed only six months before. For a telegraph road you must have a width of that kind to keep your posts clear.

I am well acquainted with the whole of the route of country taken for the telegraph lines from Kurrachee to Constantinople, and also with the route of the Australian lines. In the case of the latter it was a free, open country compared with Africa, with plenty of means of conveyance, with abundance of wood to hand, and the line once constructed, there was little or nothing to interfere with it, and yet that line cost £160 per mile. What then would be the cost through Central Africa? How are you to provide carriage of materials? Horses and oxen will not live there: no cattle will live there. There is no means of conveyance of any kind but coolies, and, if iron poles were used, a line of this length would require 133,500 coolies for the carriage of materials, and as food is not to be purchased in many parts you would be obliged to carry all such supplies from the coast. Where are you to get that number

of coolies? and as for this work coolies cannot be obtained in gangs of more than 100 at any time. I should like to know how long it would take to distribute the stores alone? To talk about an overground line in opposition to a submarine, one must be mad to do it.

The CHAIRMAN: May I ask from where to where you were carrying out this telegraph?

Mr. MAYES: I was engaged in constructing roads under the circumstances I have mentioned. There is a party of gentlemen in England who are expecting to have the whole of that part of the country conceded to them by the Sultan of Zanzibar. Their idea, I believe, is to form a company similar to the old East Indian Company if they can come to terms; and the idea is to send out traction engines for the means of transport, going from Der-es-Salome to the north end of Lake Nyassa, and then enter the south end of Lake Tanganyika, and to keep up intercommunication by small steamers. The distance to Lake Nyassa is about 400, and there are about 150 miles between the two lakes. Malaria prevails all over the country. I was myself down with fever fourteen times during the 12 months I was there last. No Englishman can stand it. Even foreign labour cannot stand the climate. I telegraphed for men to be sent to me and seven Bombay men were sent down from Aden, three of whom died in the first two months, and the other four I had to send back immediately, one dying before he reached the coast. Captain Muncreff, R.N., who went with me was soon taken ill: I had eventually to return from the same cause, and my son is coming home by the next mail. Since he has recovered, and remains there.

DR. MANN (responding to the Chairman's invitation to speak on the subject before the meeting) said that the Chairman was aware he always came to the Telegraph Engineers to learn, and not to speak; and he had learned much that interested him as an old South African, very much on this occasion, that portion of Mr. Sivewright's communication which dealt with the way in which local difficulties and obstacles had been overcome was to him full of suggestion and instruction. The evidence furnished by the map of the great progress already made by telegraphic lines in South *Africa*, and of the admirable way in which the securing of alternative

routes to meet possible emergencies has been accomplished, was to him matter of some surprise, as well as of gratification. It was tantalising, however, just at this time to note on the map that the communication which extended from Cape Town, stopped at Stanger, not more than 80 miles from the spot to which Colonel Pearson was shut in at Ekowe in Zululand by his beleaguerment of savage foes. The advocacy of Mr. Sivewright of an overland line across Africa to bring the Cape into relation with the European system was assuredly of the deepest interest and utmost moment. It was certainly a consummation to be devoutly wished by all friends of Africa. But the difficulties which this undertaking would entail must not be underrated; they could be conquered, but they were great; and their conquest would require time. In the present emergency the Marine Telegraph for communication with South Africa could most quickly be brought into work, and therefore it must be proceeded with at once. But if Mr. Sivewright's principle of the importance of alternative routes was sound, and of universal application, then it might surely be permitted to hope that the alternative route in South Africa, after the sea cable had been laid, would be the overland route. But even apart from this it should not be overlooked that when once communication with England is established by a sea cable, this becomes the best of all reasons why the land lines should be pushed forward into the interior, to increase the service which the direct communication with the great centre of civilisation is capable of rendering.

Mr. Treuenfeld: With regard to Mr. Sivewright's question "Why the first constructors of the lines at the Cape did not carry the telegraph along the high roads instead of at a certain distance from them?", it will be well to consider that when the first lines were constructed 10 or 15 years ago, high roads at the Cape and elsewhere consisted of mere tracks for bullock carts. These carts carry at times a load 15 feet high and had the telegraph line been erected along these tracks, it must have been destroyed. Referring to the question as to whether wood poles or iron poles are the best for the colonies: if considered as an economical question I should say that where timber is at hand wooden poles have the advantage, but where efficiency and regular working of the line have to be

maintained, iron poles are in my opinion the best. In fact, in colonies and all places where reliable men cannot be obtained as linemen, the strongest materials should be used for the lines despite the higher first cost, and I am glad to have heard from Mr. Sivewright that the lines at the Cape, which were constructed many years ago of strong iron poles did not show any signs of deterioration when he arrived there. Now that the war is raging it would be interesting to know whether the telegraph line has been prolonged to the seat of war. It would seem from the observations of Dr. Mann that the terminal station is still at Durban.

Dr. MANN: It is prolonged to the coast line. It goes through Verulam, and I believe to Stanger. It is only a distance of 30 or 40 miles to the scene of operations.

Mr. MAYES: With regard to iron and wooden poles, iron is better for South Africa than wood, and cheaper. The line from Graffreinet to Murraysburg, to Beaufort, Beaufort West and Murraysburg to Richmond and from there to Victoria West I put up, myself, with iron poles, and that cost the colony £100 per mile: The line from Queen's Town to Dordrecht on to Aliwal North and to Burghersdorp is of wooden poles. For such wooden poles as I have used I had to pay 29/6 for sneezewood poles. That is a tree which grows straight for only 6 or 8 feet and then throws off two branches. It makes one of the ugliest looking lines in the world. As to the durability of the sneezewood poles there can be no question. The only thing against them is the unsightliness of the line, but as the sneezewood can only be obtained in the vicinity of the coast, carriage into the interior forms a heavy item of cost. That line cost about £85 per mile.

Major WEBBER asked whether the line from Heidelberg to Pretoria had been constructed.

Mr. MAYES: It has been from Graffreinet to Murraysburg and to Beaufort West with Murraysburg, to Richmond and to Victoria West.

Mr. SARGEAUNT (Crown Agent for the Colonies): I do not know you should invite me to make any remarks. I need not say not an expert in matters of telegraphy; but though I am not it to propose a vote of thanks to myself I think I am entitled

to some credit that you have had such an interesting paper read to you as you have this evening. When I look at that map in front of me, and at the small red lines over that part of the country, I recollect the time when not one-tenth of those lines could have been drawn; indeed, I was in South Africa some twenty-six years ago when there were no telegraphs in that country. The Cape Government some short time back found that it required some one with the necessary experience and skill to manage its telegraph affairs, and accordingly asked my Department to select a suitable man. On reference to Mr. Preece he suggested Mr. Sivewright. This gentleman, directly I saw him, struck me as being just the man we wanted, and it is very gratifying to me to feel that he has proved himself to be so, an opinion shared by Mr. Molteno, the late Prime Minister, by Sir Bartle Frere, and by Mr. J. Gordon Sprigg, the present Prime Minister, and I was not a little pleased when I heard that the Cape Government and Natal had federalised, so to say, in these matters, so far as to make Mr. Sivewright's services available not only for the Cape Government, but also for Natal. Mr. Sivewright as you are aware, is at the present moment acting in Natal.

When the project was first brought under my notice of a land line, I will not say a line through Central Africa, but when a land line was brought under my notice I shook my head and said "Who could propose it?" When I went to the Cape about 18 months ago I was sceptical as to the possibility and probability of such a scheme. I went to Pretoria, and the further I went, and the more people I talked to, and the more information I gained, the less I could hold to my first opinion, and I believe there are some in this room who will live to see such a line.

Mr. SPAGNOLETTI: The difficulty and cost of land transport in this Colony has been spoken of as very great, but as some information has also been given as to the suitable fir timber for telegraph poles which can be obtained from the northern forests, could not these be used if the objection of their being so full of sap which causes their premature decay be overcome? This might be done and the duration of the life of poles from such timber would in my opinion be prolonged for years by the process of kyanizing;

it is a process particularly adapted for such timber, but I do not think it is a system which has been much investigated by Telegraph Engineers, although the process has been largely used for timber used on railways. In 1866 I erected a line of about 56 miles with poles so treated, and they have lasted well for 13 years, and many are at present quite sound. It is not so good a preservative as creosote, but as creosoting in South Africa is quite out of the question, kyanizing in my opinion is the next best, and will be found a very valuable substitute, and in addition to the preservative qualities it would, I believe, protect them from the white ant.

With regard to the effects of lightning, I believe, a great protection against it would be found in the use of Mr. Oppenheimer's insulator, the form of which appears very simple, it consists of a screw bolt pointed on the top which is screwed into a porcelain cone open at the top, a cover of iron is put over this to which the line wire is bound, and the top of the iron cover comes close to the point on the top of the bolt which is connected to the earth, and allows an escape of the current from any violent discharge that the wire may receive.

Major WEBBER: There is one point which has not been discussed this evening, although it was naturally touched upon by Mr. Sivewright. He said very little as to the use to which the telegraph in Kaffraria was put in the war of 1878, though he tells us that three or four miles of the line were destroyed by the Kaffirs. It would have been very interesting, from what we know is going on now in South Africa, if he had been able to tell us, whether that telegraph line had been of use in the late military operations, and to what extent. It is possible that those operations may have been similar to those in the Kaffir war of 1851–52 when little more was done than making patrols into the Kaffir country, and burning and destroying the kraals and crops of the Kaffirs. Probably they were confined principally to the forming of outposts, fortifying them, and holding them until the Kaffirs were brought to terms. Under such circumstances it might have been considered undesirable to spend so much money for telegraph lines to those parts, especially (as we are aware) because the material which was to be had in the colony *would be much too* heavy for such a purpose, namely for making light

temporary lines from one military post to another. We next come to the present war, and the desirability of the use of the telegraph in connection with it. The question would naturally be asked by telegraph engineers (and it has been asked in the press) why were not telegraph lines constructed to communicate with the forces that have lately invaded Zululand under Lord Chelmsford? Our answer is the reason I have already given, namely, that the material now in the colony is too heavy for such a purpose: and therefore, though Mr. Sivewright, as we have been told, is at present in Natal, we may exonerate him from responsibility in that matter.

Mr. SERGEAUNT: The material in the colony would not I think be sufficient.

Major WEBBER: I have been informed that it was sufficient for 280 miles, but useless on account of its weight.

Mr. SERGEAUNT: If you take down the present telegraphs; yes.

Major WEBBER: I understood, not erected.

Mr. SERGEAUNT: I think not for 280 miles.

The CHAIRMAN: The greater portion of that was sent out for the line from Pietermaritzburg to Utrecht.

Major WEBBER: It is curious in connection with this subject that the telegraph has only been asked for by the General commanding the army a considerable time after he had commenced operations, and also after the check which our army unfortunately received. Every one is quite aware that our army advanced in three lines, one from Lower Tugela, another from Helpmakaar, and the third entering from the western side of Zululand about Utrecht.

I have no doubt most of us are aware that there is no military error so great as to divide forces in presence of an enemy sufficiently strong to overwhelm any one or two of the divisions, but, as late wars have shown us the telegraph can convert, what otherwise would be an error, into a perfectly safe operation. It is not for me to criticise the operations of which we have had up to the present time, so meagre an account, but it is certain no telegraph was used for connecting even the basis of those three columns. Of course, the question would be, what would have been the result of having employed a telegraph? how would it have prevented the disaster that lately occurred? I think one may safely say had telegraphs

been erected, so as to communicate between all these columns, even within Natal, and up to the Zulu frontier, such telegraph would have made the operations which otherwise were fore-doomed to failure comparatively secure; but of course no telegraph could have compensated for the disregard of all well known military rules in bush warfare which led to the disaster at Isandlhana.

You are aware that a telegraph is being sent out, that is, what is called a military field telegraph which will cover about 80 miles. Twenty miles of that will be cable to lie on the ground, about twenty miles of light wire on poles, and the remainder is of wire for which I believe it is expected poles will be found in the country.

There is one point in connection with this field telegraph which is an advantage in the face of what Mr. Sivewright has told us as to the expense of carrying material; it takes sufficient to carry all its *materiel*. It is, however, said that wheel transport of every kind will not be able to move rapidly in that country, and that it would have been wiser to have taken pack animals.

My own view is, that the field telegraph of the future, for all useful purposes, both for every temporary and also for lines of a more permanent character, should be transported as is done by the Spaniards on pack animals. About 250 mules with a telegraph line weighing from 8 cwt. to 11 cwt. per mile would have been a really useful force to have sent to the colony, and if that were done now, no doubt the columns of the army could be kept in perfect communication with their bases at all times.

The greatest difficulty that is experienced is in connection with the ground cable portion of the line, particularly in an uncivilized country, inasmuch, as in savage warfare it is much more difficult to exclude the enemy from the field of operations than in the case of civilised warfare, and thus the lines are more liable to intentional injury.

In consequence of the numerous applications for copies of Colonel Bolton's paper on "Historical Notes on the Electric Light," it has been thought desirable to print the Proceedings of the Seventy-ninth Ordinary General Meeting, held on May 14th, in this number. The proceedings for the previous Meetings, numbers seventy-seven and seventy-eight, will form No. 28 of the Journal.

The Seventy-ninth Ordinary General Meeting of the Society was held on Wednesday evening, May 14th, 1879, at the Institution of Civil Engineers, 25, Great George Street, Westminster, the President, Lieut.-Col. BATEMAN-CHAMPAIN, in the Chair.

After the preliminary business had been transacted—

The PRESIDENT said: The principal paper this evening is one by our honorary secretary, Colonel Bolton, which is entitled, "Some Historical Notes on the Electric Light." A day or two ago we received a communication which is of considerable interest, and which will be read first, but this evening there will be no discussion on this short paper. It is entitled, "Notes on Earth Currents," by Mr. W. Ellis, of the Royal Observatory, Greenwich. Before having this shorter paper read, I should like to make an announcement. This is our last meeting, as I daresay you are aware, till the 12th of November next, but I hope I shall meet you again at the Society's Conversazione, which is to take place on or about the 18th of June. The Conversazione this year will have a somewhat special character, as we hope to entertain our friends the delegates from all the European and other countries at the International Telegraph Conference, to be held this year in London. At that Conference, which will be held in one of the galleries of the South Kensington Museum, we hope to collect a display of telegraph and electrical apparatus of all kinds, thereby making it a fairly comprehensive exhibition, but in order to do that we must ask the assistance of every member of the Society. Any one who can send articles of interest, or induce others to send them to this exhibition, will receive our hearty thanks, and we feel sure this intimation will not be lost upon the members.

The SECRETARY then read the following paper :—

NOTE ON EARTH CURRENTS.

By WILLIAM ELLIS, F.R.A.S., of the Royal Observatory, Greenwich.

In offering, at the present time, this Note to the Society of Telegraph Engineers, my object is to draw attention to the circumstance that during the next few years we may look forward to earth currents becoming much more active than they have been for several years past. It seems desirable, therefore, considering the present extension of telegraphs throughout the world, that this should be made generally known, and that, if possible, some instructions should also be framed with the view of securing greater uniformity than heretofore in any observations that may be made.

It is probably known to many members of the Society that the spots nearly always visible, more or less on the sun's surface, are subject to periodical variation both in number and magnitude, the successive epochs of maximum and the successive epochs of minimum being separated by an interval of about eleven years. There is a little irregularity in the progression, but of the existence of a somewhat irregular period of the kind there appears at present to be little doubt. One peculiarity in connection therewith is that the epochs of minima are not equidistant from the adjacent epochs of maxima. For instance, epochs of minima occurred in the years 1843, 1856, and 1867, and epochs of maxima in the years 1848, 1860, and 1870. The interval from minimum to maximum is thus about four years, and that from maximum to minimum about seven years. The last period of maximum having occurred in 1870, the next will probably occur about the year 1882. We have certainly of late experienced some very quiet years, and may at any time expect to see a renewal of solar activity.

But how does this, it may be asked, bear upon the question of earth currents? I will endeavour to explain. It appears to be tolerably well established that magnetic activity (as measured by the diurnal magnetic variations) increases and decreases with the

increase and decrease of sun spots. When sun spots are at a maximum there is greater magnetic activity, and when at a minimum, a corresponding lesser magnetic activity. But it is found at Greenwich that, when there is great magnetic activity, earth currents are also more active. Therefore, as we approach the next epoch of solar spot maximum, we may expect not only increased magnetic action, but also greater earth current action. In order to afford some estimate of the amount of disturbance that may be looked for, it may be useful to state what was observed at Greenwich about the time of the 1870 maximum. As a scale of comparison, we may refer to the magnetic disturbance of 1878, May 14, which, although (as observed at Greenwich) one of no very great magnitude, was yet sufficient, as mentioned by Mr. W. H. Preece, in a communication to "Nature," Vol. 18, page 668, to cause difficulty in the telegraphic working at Haverfordwest, in consequence of the accompanying earth currents. Taking, then, the general magnitude of the disturbance on this day as a unit of measure, I have set down in the following table the number of days in each of the years 1869, 1870, and 1871, on which magnetic disturbances occurred at Greenwich equal to and greater than that of 1878, May 14, adding, for comparison, similar information for the years 1877 and 1878 (both quiet years, and years near to a minimum sun spot epoch).

Year.	Number of days on which Magnetic Disturbances occurred at Greenwich.			Total.
	Equal to	Greater than	Much greater than	
	that of 1878, May 14.			
1869.	38	17	2	57
1870.	20	12	9	41
1871.	18	9	10	37
1877.	5	0	0	5
1878.	4	1	0	5
No. of Column.	1	2	3	4

These numbers show the paucity of disturbances in recent years, as compared with their much greater frequency, as well as greater magnitude, in the years near to the 1870 sun spot maximum. In the case of the larger disturbances (those included in columns 2 and 3), the disproportion is very great, 59 in the years 1869, 1870, and 1871, against 1 in the years 1877 and 1878. It seems desirable, therefore, that, as far as may be possible, advantage should be taken of the opportunities which the approaching years are likely to afford for the systematic observation of earth currents; and if those who have the charge of telegraphic lines in different parts of the world were instructed to make observations on one uniform plan, valuable results might possibly be obtained.

There is one practical aspect of the question which may have already been sufficiently well considered, and, if necessary, provided for; if not, it is one likely to be specially interesting to telegraph engineers. I refer to the interference of earth currents with actual telegraphic work. The last few years have been very free from magnetic disturbances and their accompanying earth currents, but a more active time is probably approaching. I am not intimately acquainted with modern telegraphic applications, but I have an impression that, during recent quiet years, new forms of apparatus and more delicate methods of working have been introduced into the ordinary telegraphic routine, and I would therefore ask whether any of the new apparatus possesses such peculiarity in its principle or construction as would render it more liable than are the older forms to be temporarily deranged or interfered with by earth currents? It has been already mentioned that, on the occasion of the magnetic disturbance of 1878, May 14, difficulty was experienced in the telegraphic working at Haverfordwest. Is this to be taken as an indication of what is to be expected when disturbances begin again to be numerous? For if so, it should be remembered that we shall then undoubtedly have many instances of disturbance equal to that of 1878, May 14, and probably not a few much greater. Are telegraph engineers and others who may be interested in telegraphic apparatus generally aware of this circumstance?

The PRESIDENT: You will allow me, gentlemen, I am sure, to express our thanks to Mr. Ellis for his paper. The subject of earth currents is a very interesting one to the members of this Society, and is now engaging the continuous attention of the Council.

The vote of thanks was unanimously accorded.

The PRESIDENT having called upon Colonel Bolton to read his paper,

"SOME HISTORICAL NOTES ON THE ELECTRIC LIGHT,"

Colonel BOLTON expressed his regret that, through a severe cold and hoarseness, he was obliged to forego the pleasure of reading his paper, and he trusted the Members would kindly allow his friend Professor Ayrton and the Secretary respectively to perform that duty for him. (Hear, hear.)

INTRODUCTION.

IN inviting your attention to these Historical Notes on the Electric Light, which must inevitably be considered a dry subject, I feel that some apology is necessary for thus occupying your time. My apology will be found in a sincere desire to promote the progress of Electrical Science, and to assist those members of the Society of Telegraph Engineers who are interested in the development of inventions: this will be best explained by a simple statement of a few facts.

In the autumn of last year the excitement regarding the electric light differed altogether from that concerning the telephone and microphone, in that these latter were genuine inventions of a very wonderful nature, and therefore merited much attention; whereas no new and startling principle of electric lighting had been lately evolved, and therefore, consequently, the excitement which caused so many people to patent something or other in electric lighting, and made the gas shares descend so rapidly in price, arose, in a great measure, from the ignorance of the public as to what had been already done, and what had been already patented in connection with electric lighting.

An inventor having got an idea into his head which was perfectly new to him, and thereby considering it an idea worthy of a patent, would probably adopt one or the other of the inevitable three courses;—(1) he would, if of a self-satisfied disposition, at once take out provisional protection; (2) or he would apply to a patent agent to know if his invention was a novelty; or (3) would make research in the Patent Office himself.

But excellent as the arrangements of this important branch of Her Majesty's service are, and great as are the facilities afforded to the public, yet I venture to express an opinion of regret, in the interests generally of those men of inventive genius from whose resources the large revenue of this department are derived, that the Abridgments of Specifications are not more closely written up and published. For instance, the last Abridgments of Specifications relating to electricity and magnetism does not go beyond 1866, and even the very specifications themselves are always months behind hand: for instance, at this date (May 14th) it is impossible to ascertain clearly the details of any patent on electric lighting later than those of last year.

Few know, except those who have tried it, the amount of labour and brain irritation which is endured by a person of an active and sanguine temperament in trying to find out whether anybody else has been before him and has forestalled his idea, which perhaps may be his one ewe lamb.

In an evil hour for myself, an idea struck me, which was not, however, patentable, although I felt sure it was novel, and I hoped that it might prove useful. It occurred to me that great benefit might be derived from the publication, in the Journal of our Society, of a paper not being a mere abstract or abridgment of patents such as are issued from time to time by the Patent Office, and appear in the professional and other newspapers, but a classification of the patents under different headings, according to the most important principles contained in them, so that if anyone has a new idea he may see at a glance whether his idea is original, and, if not, what exactly has been done in the same direction. Notwithstanding the desirability of having such a means of *reference* at the command of the members of our Society,

applicable to all those matters in which the Electrician and Telegraph Engineer are interested, I found that the task would be too great and the results too voluminous to be undertaken in the way which at first suggested itself to me, except spasmodically, and as appertaining to any given subject which might command the particular attention of the moment. I therefore at first determined to confine myself to the effects of Voltaic Electricity, but when on further thought I considered that these effects are threefold—Physical, Chemical, and Physiological—and that under the first head alone are included the electrical, luminous, calorific, and electro-magnetic phenomena of the circuit, I drew in my horns, and resolved not to attempt anything beyond that subject which occupied at the time the greater amount of public attention, viz., Electric Lighting; leaving it to other times, and I hope to other members of the Society with more ability and time than I possess, to carry out the project of abstraction and classification of patents appertaining to applied Electrical Science, if, peradventure, this initiative attempt meets with approbation.

Hence this paper on electric lighting, which is not a mere abstract of patents, since it endeavours to arrange the inventions in characteristic classes, in order to show how one patent led to another, and, also, how the same idea has been patented over and over again by different people.

The compilation of this paper, in which Professor Ayrton has given me the most valuable assistance, has been the work of some months, and the task has been no easy one, since many of the specifications referred to are very long, occupying in some instances 20 to 30 pages of close printing, and illustrated with numerous drawings, to which reference had to be made. Very often, amidst a great mass of ideas which time has shown are quite useless, is hidden away one idea to which the inventor attached but little importance at the time, but which now is known to be of great value, and would make the first inventor famous were it only known that the idea was due to him; consequently, one of the objects I have had in view in the compilation of this paper has been to search out these valuable fragments from the mass of comparatively useless suggestions by which they were surrounded,

and the subject-matter which follows will prove the previous existence of some patents, the credit for which has been given to subsequent patentees.

Great facility has been afforded in the preparation of this paper by the circumstance of our having in the library of the Society copies of all patents on Electricity and Magnetism, from the earliest period to the present date, which have been given to the Society by Her Majesty's Commissioners for Patents, and the value of this gift cannot be too highly appreciated.

The notes have been arranged in subdivisions and sections, the first subdivision being on Machines, the second on Lamps, and at the end will be found some Patents for Impossible Results.

I have, as far as possible, endeavoured to illustrate by diagrams the *characteristic* features of *each* class of patents on Electric Lighting, and Dynamo and Magneto-Electric Machines.

SUBDIVISION No. 1.

DYNAMO AND MAGNETO ELECTRIC MACHINES.

SUBDIVISION No. 2.

LAMPS.

When two names in the following list are joined by "and," it means that the patent was a joint invention of the two or more people whose names are specified. But when two names are joined by a dash (—), it means that the second name is that of the English patent-agent acting on behalf of the foreign inventor, whose name is placed before the dash.

Sentences in italics and between brackets are notes now added, explanatory of the present classifying of the patents.

Subdivision No. 1.
DYNAMO AND MAGNETO ELECTRIC MACHINES.

I.
DISCOVERY OF MAGNETO-ELECTRICITY, AND EARLY MACHINES.

1831. Faraday produced an electric spark by the sudden separation of a coiled keeper from a permanent magnet; and found also that an electric current existed in a copper

plate rotated between the poles of a magnet. He next discovered that a current was induced in a hollow coil of wire when a permanent magnet was introduced or withdrawn from the coil; and that the effect was increased by substituting an electro-magnet for the permanent one.

1832. PIXII. A large horse-shoe magnet was set in rotation round a vertical axis beneath a similar horse-shoe armature of soft iron, the two branches of which were surrounded by a silk-covered wire.

1833. SAXTON placed the whole apparatus horizontal, fixed the compound horse-shoe magnet, and rotated the armature in front. One terminal was the axle of the machine, and the other a disc insulated from it and rotating in mercury.

1834. E. M. CLARKE added two armatures, one wound with thick wire and the other with thin; to be used as the external resistance was small or large.

II.

CONTINUOUS CURRENTS.

[*This section contains the most important patents for converting the alternate currents produced by any one of the moving bobbins into currents of the same direction in the circuit external to the machine.*]

1835. SAXTON also substituted two armatures, one wound with thick and one with thin wire. As the currents produced by a bobbin were alternate, he suppressed one set of currents in order to obtain a continuous current.

1836. E. M. CLARKE placed the magnets vertically to avoid injurious shocks in the working of the machine, still, however, revolving the coils about a horizontal axis. Added a commutator, which made the currents in the external circuits all flow in the same direction, but not with uniform strength.

1841. WHEATSTONE, 9,022. "Improvements in Producing, Regulating, and Applying Electric Currents."

To obtain a perfectly uniform current from a magneto-electric machine, he combines several distinct

machines, the bobbins being all turned by one shaft, and all the wires leading to one commutator, the relative position of the bobbins and magnets in the different machines at any moment being such that the sum of the currents is a constant. The machine employed is a kind of compound Clarke's machine. Proposes the use of resistance coils.

1857. HOLMES, 1,998. "Improvements in Magneto-Electric and Electro-Magnetic Machines."

Two sets of revolving bobbins and two commutators joined up together, so that the sum of the currents is a constant.

1870. GRAMME AND D'IVERNOIS, 1,668. "Improvements in Magneto-Electric Machines."

Produces a continuous current without the old form of commutator, by using the Gramme ring. Revolves either the bobbin or the permanent or electro-magnets.

1877. LONTIN—A. M. CLARK, 4,893. "Improvements in Dynamo-Electric and Magneto-Electric Machines."

Moves a magnet parallel to an iron ring, on which are wound coils of wire, and so produces a continuous current without a commutator.

III.

ELECTRO-MAGNETS, INSTEAD OF PERMANENT MAGNETS, AS INDUCERS.

845. WHEATSTONE, 10,655. "Improvements in Electric Telegraphs and Apparatus relating thereto," etc.

Substitutes voltaic magnets instead of permanent magnets in magneto-electric machines.

352. WATT, 834. "Improvements in Obtaining Currents of Electricity."

In the magneto-electric machine electro-magnets are used instead of permanent magnets, and the armatures have a reciprocating as well as rotatory motion, in order to diminish residual magnetism.

IV.

BOBBINS OF THE "KING" TYPE, COMMONLY KNOWN AS THE "WILDE."

[*The characteristics of this form of machine are that the coils are fixed on a disc, the axis of the coils being parallel to the axis of rotation of the disc. This is exemplified in figures* 1 *and* 2, *which show the elevation and plan of the machine patented by Millward, No.* 13,536 *of* 1861; *and which have been given as being more distinct than the illustration accompanying King's patent, No.* 11,188, *which is the first sample of the type, or the illustration accompanying Shepard's patent, No.* 13,302. *The specimens of this type best known are the machines of Holmes and Wilde.*]

1846. KING, 11,188. "Improvements in the Production of Magneto-Electricity."

The principle of the machine consists in revolving between the poles of permanent magnets arranged radially, a disc having near its edge bobbins with their axes parallel to the axis of rotation.

Winds round the iron cores a continuous flat strip of copper like a surgical bandage, inserting cotton soaked in beeswax and rosin between each layer, and putting the edges of the strip next the core.

Collects the currents from the separate bobbins with separate springs, to allow of subdivision if necessary.

To prevent neutralising currents being induced in the brass or other metallic plate which forms the wheel carrying the armatures, a saw-cut is made from the edge to the hole in which the armature is inserted.

Attaches a soft iron bar to the inducing magnets, so that they may each act a second time on any armature during each revolution.

1850. SHEPARD, 13,302. "Certain Improvements in Electro-Magnetic Apparatus suitable for the Production of Motive Power, of Heat, and Light."

Four electro-magnetic bobbins, with their axes parallel to the axis of rotation, rotate between the poles of per-

manent horse-shoe compound magnets, the plane of which contains the axis of rotation.

Proposes various plans for moving the bobbins, oscillating motion, etc., etc.

Suggests passing the current produced by the magneto-electric machine through induction coils, or into "current condensers made of a number of very thin metallic bars, varnished and wound round a roller, or hollow cylinder, made of thin well annealed sheet-iron."

1851. MILLWARD, 13,536. "Certain Improvements in Electro-Magnetic and Magneto-Electric Apparatus."

Many bobbins fixed on a disc, with their axes parallel to the axis of rotation, revolve between the poles of many horse-shoe magnets. [See figures 1 and 2.]

1852. SHEPARD, 14,197. "Electro-Magnetic Apparatus for the Production of Motive Power, Light, and Heat."

A disc carrying flat bobbins, with their axes parallel to the axes of rotation of the disc, rotates past permanent compound horse-shoe magnets arranged radially in a flat circle in a plane parallel to that of the disc.

Suggests, as an alternative, fixing the disc and rotating the magnets.

The apparatus by means of a commutator produces "continuous currents," but the strength keeps varying from a maximum to nought.

1853. SHEPARD, 1587. "Improvements in Magneto-Electric Apparatus suitable for the production of Motive Power, of Heat, and of Light."

Consists of an improvement on patent No. 14,197 of 1852, the general principle remaining the same, the main change being in the joining up of the sixteen coils comprising one revolving disc, so as to produce four distinct currents in four separate circuits, each current being an alternate current.

1856. HOLMES, 573. "Improvements in Magneto-Electric Machines."

Flat coils arranged round a disc, with their axes parallel

to the axis of rotation, are carried past flat horse-shoe permanent magnets arranged radially in a parallel plane: Non-lubricated commutator:—springs pressing on the commutator are furnished with little wheels.

The points of contact of the conductors with the commutator are varied by a governor as the speed of the machine changes, in order to avoid sparks arising from the induction in the moving bobbins taking place at different distances from the poles of the magnets when the bobbins are rotated at different speeds.

1857. HOLMES, 2,628. "Improvements in Magneto-Electric Machines."

A large number of horse-shoe permanent magnets and bobbins on the revolving disc employed, so that a low speed may be used. The bobbins pass *very near* the poles of the magnets. A projecting guard ring is attached to each disc to prevent the possibility, in case of derangement, of the cores of the bobbins coming into contact with the magnets. The ends of the cores are also bevelled for the same object.

1857. SHEPARD, 2,987. "Improvements in Magneto-Electric Machines."

The various bobbins are joined up by a special arrangement, so that as far as the commutator is concerned they are practically equivalent to one bobbin, thus making the commutator very simple.

1859. NEWTON, 1,647. "Improvements in Magneto-Electric Machines."

Compound permanent magnets, each complete magnet forming the spoke of a wheel, rotate. The coils have their axes parallel to the axis of rotation, and their ends opposite the circumference of the circle described by the magnets. The reversal of the current is produced by an arm set in vibration by electro-magnets, the polarity of which is reversed by the reversal of the main current in the main machine.

1866. WILDE, 3,209. "Improvements in Electro-Magnetic and Magneto-Electric Machines." *Provisional protection only.*

A wrought-iron disc carries an even number of cylindrical bars, arranged at equal distances around the circumference, and parallel to the axes of the disc. The bars form the cores of electro-magnets, having alternately north and south poles. Two of these discs fitted with compound electro-magnets are mounted with the centres of the discs in the same straight line, the bars of the one disc having poles opposite to those on the other disc. Between these two discs revolves concentrically a disc carrying as many electro-helices as there are cylindrical bars on either of the fixed discs. The stationary electro-magnets are excited by an auxiliary magneto-electric machine, and alternate currents are finally produced.

Proposes a commutator to revolve so fast that, although only made of two pieces, it reverses the final current as often as there is an inversion of the current in the machine. This is to replace the form of commutator in which there are as many segments as there are alterations of the current in one revolution of the wheel carrying the electro-helices.

1867. Mennon, 1,611. "Improvements in the Construction of Magneto-Electric Batteries."

A number of coils, with their axes vertical, are placed round the circumference of a horizontal wheel. Several such wheels at distances from one another revolve round a vertical axis. A number of permanent bar magnets are fixed vertically, so as to form the sides of a long cylinder, of which the wheels may be regarded as sections. All the magnets in any one vertical line have their poles arranged in the same way.

The coils in one of the wheels are a little in advance of those in another, so that if at any moment the current given by the coils in one wheel is at a minimum, that given by the coils in some one of the other wheels will be at a maximum. The total current therefore given by the machine is nearly constant.

1877. Johnson, 3,743. "Improvements in Magneto-Electric Machines."

The currents from a machine of the Holmes type are conveyed to a metallic disc by a rubber pressing on the disc. The currents from any particular set of bobbins may be conveyed to two discs, and so the whole current in the machine may be divided up.

V.

MOVING PERMANENT MAGNETS ACTING ON FIXED COILED PERMANENT MAGNETS.

1843. SCHOTTLAENDER, 9,982. "Electro-Deposition of Metals on Felted and other Fabrics."

Magnetic battery. An internal crown of horse-shoe permanent magnets revolves inside an outer crown of permanent horse-shoe magnets wound with insulated wire. [This is shown in figure 3.]

VI.

INDUCING ELECTRO-MAGNETS ACTED ON BY THE CURRENT PRODUCED BY A MAGNETO-ELECTRIC MACHINE.

1854. HJORTH, 2,198. "An Improved Magneto-Electric Battery." *Provisional protection only.*

The coiled armatures revolve between the poles of fixed cast-iron permanent magnets and of fixed electro-magnets, "in such a manner that the currents induced in the coils of "the revolving armatures are allowed to pass round the "electro-magnets; consequently the more the electro- "magnets are excited in the said manner, the more will "the armatures be excited, and more electricity of course "induced in the respective coilings; and while a mutual "and accelerating force is thus produced in this manner "between the electro-magnets and the armatures, an "additional or secondary current is at the same time "induced in the coiling of the electro-magnets by the "*motion* of the armatures, the said current flowing in the

"same direction as that of the primary current, after having "passed the commutator."

A A, figure 4, are the permanent magnets, *B B* the electro-magnets, and *C C* the revolving electro-magnetic bobbins in which the current is generated.

He adds, "The permanent magnets may be coiled like "the electro-magnets, which also will serve to make them "more permanent."

[*This apparently includes the principle subsequently rediscovered by others.*]

1855. HJORTH, 806. "An Improved Magneto-Electric Battery."

An improvement on No. 2,198 of 1854, but without change of principle, the chief alteration consisting in the substitution of electro-magnets of concentric coiled tubes.

1863. WILDE, 3,006. "Improvements in the Construction and Working of Electric Telegraphs," etc.

Uses for "producing the powerful currents necessary to transmit messages through long submarine cables," a Siemens armature revolving between the poles of a long horse-shoe electro-magnet, excited by means of a battery, or by means of the small magneto-electric machine with permanent horse-shoe magnets, described in No. 516 of 1863. (See Sec. VII., Machines.)

1865. WILDE, 2,762. "Improvements in the Construction and Working of Electric Telegraphs and in Apparatus connected therewith."

Whenever the armature of the small magneto-electric machine mentioned in the last patent is at the dead point, or the machine is producing no electricity, a short circuit is made between the two wires going from the smaller machine to the electro-magnet of the larger.

1866. CORNELIUS AND SAMUEL A. VARLEY, 3,394. "Improvements in the Means and Apparatus for Generating Electricity." *Provisional protection only.*

Two electro-magnets of a horse-shoe form and two bobbins are so arranged that when the bobbins rotate they act simultaneously, the one between the poles of one magnet,

and the other between the poles of the other. By means of a commutator, the whole is joined up into one continuous circuit, and produces a constant current. Starts the action by sending a current through the electro-magnets, in order to produce an initial quantity of magnetism.

To prolong the contact between the bobbins and the poles of the electro-magnets, they arm the electro-magnets or bobbins with horns of soft iron.

Sometimes uses one iron bobbin, and makes it oscillate between the poles of two electro-magnets by mounting the bobbin on a lever moved by means of an eccentric or crank.

For currents of a definite amount of force for ringing bells, etc., they use permanent magnets, with a bobbin moved by a lever.

[*In January*, 1867, *Dr. Werner Siemens communicated to the Academy of Sciences, Berlin, and in February*, 1867, *Dr. William Siemens communicated to the Royal Society, London, the plan of constructing a dynamo-electric machine, such, that the current produced by the machine was that which was employed to magnetise the inducting electro-magnets, and which could be started by means of the residual magnetism contained in the soft iron, thus using no permanent magnets nor auxiliary currents. About the same time Sir Charles Wheatstone published a similar idea.*]

1867. SIEMENS, 261. "Producing Electric Lights at Sea."

Magneto-electric machine without a battery or permanent magnets. The current which magnetises the electro-magnets is that which is produced by the machine itself. Uses either his armature described in the provisional specification No. 2,107 of 1856 (see Sec. VII., Machines), with two horse-shoe electro-magnets, or else a rotatory magnetic ring, with a small piece of brass between its poles, turning inside a number of bobbins enclosing it, and so arranged that only those four which are at any moment nearest the poles of the magnet are in circuit.

[*The plan of revolving a magnetic ring inside coils of*

wire is like that employed by Roberts in his patent, No. 14,198 *of* 1852, *Section X.*]

1867. WILDE, 842. "Electro-Magnetic and Magneto-Electric Induction Machines."

1. Proposes a form of double magneto-electric machine, one worked with permanent magnets for producing the current to excite the fixed inducing electro-magnets of the other machine.

2. To overcome the objection of the current producing the light having to pass through the inducing electro-magnets, he proposes that a certain number of the revolving coils shall be used to produce a continuous current, magnetising the inducing magnets, while the remainder produce the light current, the whole being started by residual magnetism.

3. Proposes to coil two wires round the revolving armature, one for exciting the stationary magnets, the other for producing the light, etc.

4. Proposes to place the small armature for exciting the electro-magnet, and the larger armature for producing the more powerful currents, between the poles of one horse-shoe electro-magnet, started by residual magnetism, or to use a Siemens armature between the poles of a long horse-shoe electro-magnet.

5. An improvement in the commutator, so that the spark usually produced in the contact spring passing from one to the other half of the commutator is prevented by or transferred to a short circuit piece revolving with the commutator.

867 CORNELIUS AND SAMUEL A. VARLEY, 1,755. "Improvements in Electric Telegraphs."

Repatents No. 3,394 of 1866, for which provisional protection only was obtained.

1867. HOLMES, 2,221. "Apparatus for Producing Electric Light." *Provisional protection only.*

Proposes to make a number of armatures revolve in front of a number of electro-magnets, so that a low speed

may be used. Employs a small portion of the wire which passes round the several armatures to magnetise the electro-magnets. No permanent magnets used, but, instead, the current from a small galvanic battery starts the action.

Winds on the electro-magnets and on the armatures a double or treble set of coils, for the purpose of increasing or diminishing the strength of the light without stopping or altering the speed of the machine, this variation in the strength of the light being necessitated by fog, etc.

1867. HOLMES, 2,307. "Improvement in Apparatus for Producing the Electric Light."

A complete specification for obtaining the results referred to in the above provisional specification.

1868. HOLMES, 2,060. "Improvements in Electro-Magnetic and Magneto-Electric Machines."

Uses, if required, cores wound with very fine wire, so as to produce ozone without the intervention of a condenser.

The magnetising coils are short circuited with the electro-magnets, and then after a time suddenly separated, and the whole current discharged in any given direction, for discharging fuses, etc.

1869. HOLMES, 1,744. "Improvements in Magneto-Electric Machines."

1. Uses split tubes, riveted into soft iron split end plates, the end plates being bolted on to the brass circular standards of the machine.

2. In a machine of twenty helices, ten alternate ones produce the current used externally, while six are coupled with a Wheatstone's bridge in such a manner that, by inserting two pins, these six helices alone may send their current through the coils of the electro-magnets, or by alteration of the pins, eight or ten helices may be used for magnetising the electro-magnets.

3. Uses electro-magnets arranged radially, with other coils arranged round the circumference of a circle, the axes of these latter coils being parallel to the axis of rotation of the machine. Revolves either one or the other.

4. A stream of cold water is made to flow through the machine to cool it.

5. Proposes that the roller, by means of which the current is led from the commutator to the external circuit, shall be in contact with two teeth of the commutator at the same time for one-fourth of the time of passing a tooth, so as to short circuit the coils and prevent sparks at the moment of reversing, or if the commutator be of the usual form, that the short circuit shall be made by means of a rubber, the object of the short circuiting being to avoid sparking. When he uses rubbers in lieu of rollers, he prefers to wind the helices which serve to magnetise the electro-magnets with six or more fine wires insulated throughout the series, and terminating in a corresponding number of insulated rubbers.

6. Proposes to have holes in the hollow boss close to the shaft, to allow air to enter to cool the machine.

1876. SAMUEL VARLEY, 4,905. "Improvements in Apparatus for producing the Electric Light." Improvements on Patent No. 1,755 of 1867.

Substitutes group of bobbins for single bobbins, and arranges the commutator in such a way, that when any group is developing least electricity it is cut out of the circuit.

The light produced acts as a shunt to the electro-magnets, so that, the less the current passing through the light, the stronger become the inducing magnets.

[1]877. VARLEY, 4,435. "Improvements in Electrical Apparatus for Lighting and other purposes."

Builds up his bobbins of iron wires wrapped with cotton or other insulated substance, and the whole surrounded with convolutions of insulated wire, the object of the arrangement being to avoid the development of electric currents in the iron of the bobbins.

For subdividing the light, uses "electric division bobbins," which are really "Camacho" magents, with many iron wires arranged in cylindrical layers, instead of using iron tubes; the wires forming the various convolutions are,

however, distinct, forming separate circuits for the various lamps. These bobbins are fixed on a shaft which has an oscillating motion in the direction of its length. Surrounding the bobbins and shaft at certain parts of its length, are hollow iron pieces, which are the prolongation of the poles of electric magnets, the alternate pieces having the same polarity. The result is that the bobbin or shaft between two of these iron pieces is magnetised in the direction of the length of the shaft, and the magnetism is constantly being reversed as the shaft is oscillated backwards and forwards.

1877. Weston—Lake, 4,748. "Improvements in Magneto-Electric Machines."

The positions of the collecting brushes relatively to the commutator can be altered by turning the disc to which they are fixed round the common axis of the machine, and fixing it with set screws.

The machine is short circuited when the instrument is running at less than a certain speed, to avoid the opposing current from an electroplating bath depolarising the magnet as the machine slows. When, however, it is going at more than a certain speed, centrifugal force opens the key, and the current produced by the machine passes through the bath.

Plans for cooling the machine by a stream of water passing through the machine.

1877. Weston—Lake, 4,903. "Improvements in Magneto-Electric Machines."

Makes a compound Siemens bobbin of a non-magnetic or a diamagnetic substance, with small radial holes to allow air to enter, and leave two longitudinal holes in the bobbin by means of which it may be kept cool.

Inside and outside the revolving bobbin are stationary electro-magnets.

In another form the bobbin is stationary, and an interior electro-magnet revolves, there being no external electro-magnets.

Suggests, as one of the ways of making the interior

magnet, a number of iron discs on a shaft, with a slight separation between any two adjacent discs. Each disc has eight flat teeth on its circumference. A coil is then wound diametrically round each disc, in each of the four spaces separated by the teeth. These teeth will, he then says, become the poles of the magnet.

VII.

BOBBINS OF THE "SIEMENS" TYPE.

1856. Siemens, 2,107. "Electric Telegraphs and Apparatus." *Provisional protection only.*

Proposes for a magneto-electric machine an armature consisting of a long iron bar cut in two long grooves and wound longitudinally with wire, to be revolved between the poles of a number of horse-shoe permanent magnets, or bar magnets, with a soft iron connecting plate, the poles in either case being cut to receive the armature.

The advantages of this form of bobbin are, first, the magnetic field in which the bobbin revolves can be made stronger than in the other form of machines; secondly, more of the wire composing the bobbin is employed in cutting the lines of force and not moving parallel to them; and thirdly, the bobbin, by revolving round its own axis, diminishes the resistance opposed by the air to its motion.

1863. Wilde, 516. "Improvements in Electro-Magnetic Telegraphs."

Rotates a Siemens armature between the poles of many horse-shoe magnets.

The commutator is formed of two cylinders of hardened steel insulated from one another, the space separating the two being spiral, so that the commutator may run more freely over the springs.

[*The bobbin, together with the commutator, is shown in Figure 5.*]

VIII.

BOBBINS OF THE "ALLAN" TYPE, BETTER AS THE "LONTIN."

[*The characteristic of this form of machine is that are radii of the revolving wheel, as shown in Figure accompanies the patent of Allan, No.* 14,190 *of* 1852.]

1852. ALLAN, 14,190. "Producing and Applying Electri

Spokes on which wire is wound in a conical for in a circle, on the circumference of which are the horse-shoe permanent magnets, the horse-shoe being arranged radially.

1869. LONTIN—LAKE, 917. "An Improved Electro-M Machine."

Four radial horse-shoe electro-magnets rotate four horse-shoe radial and larger electro-magnets. the smaller electro-magnets are employed in produc current to magnetise the larger, and the remaining two electro-magnets produce the current used outside.

Suggests using steel as the sole plate of the electro-magnets, so that they may always retain su magnetism to start the current.

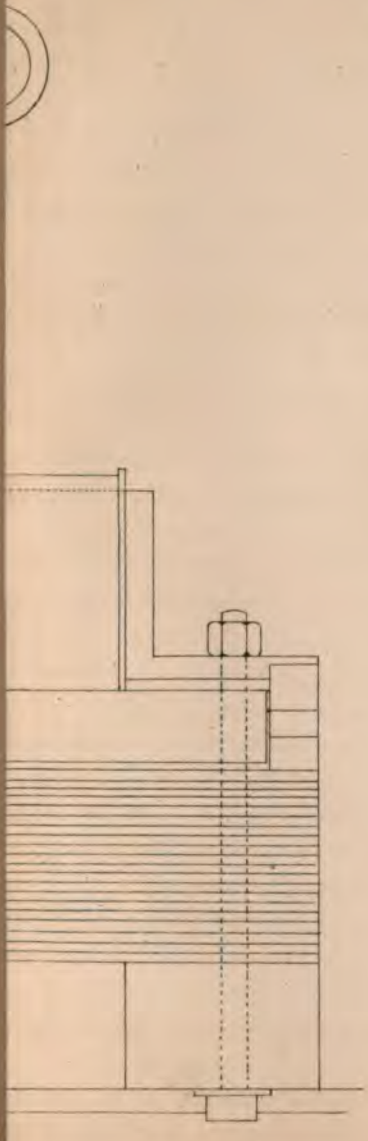
1875. FULLER—LAKE, 3,364. "Improvements in Ma Electric Machines."

A number of electro-magnets, arranged in the fo spokes of a wheel, rotate in a vertical plane betwe tangential armatures of two vertical electro-magnets. armatures are tapered so as to form a considerable po of the circumference of a circle.

To excite the stationary electro-magnets, uses eit battery, a separate magneto-electro machine, or some o revolving coils.

1875. BERTIN, 4,311. "Improvements in Dynamo and Magn Electric Machines."

Has separate commutators for all the induction so that the instruments may be joined up so as to have internal resistance.



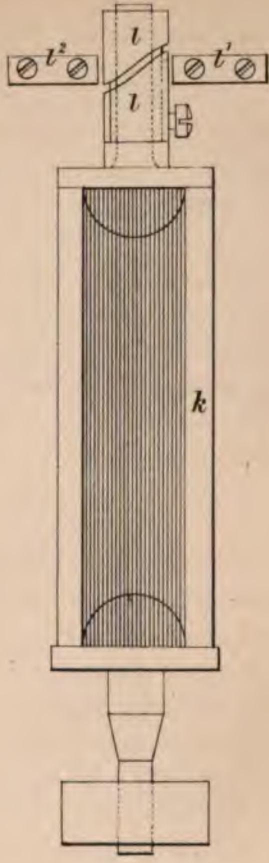


Fig. 5.

876. LONTIN—A. M. CLARK, 386. "Improvements in Dynamo-Electric Machines."

1st. Inducing coils rotate, induced are stationary, so that if there be a number of stationary coils in which the current is induced, the currents from the different coils can be employed separately or collectively, without the use of any collectors. One special application is the producing of a number of electric lights from the same machine.

2nd. Increases the length of the inducing electro-magnets so that one or more insulated wires may be placed thereon, by means of which currents can be obtained either from the revolving coils or from the extra coils on the inducing magnets, when the other coils revolve.

1876. ZANNI, 2,821. "Improvements in Magneto-Electric Machines."

Uses a number of electro-magnets fixed like the spokes of a wheel, but widening out towards the circumference so as to occupy nearly the whole area of the circle. This widening out is produced, not by enlarging the diameter of the core, but by winding more wire on the electro-magnet near the circumference.

[*The figure closely resembles No.* 6 *accompanying Allan's patent, No.* 14,190 *of* 1852, *the chief difference being the addition of tangential iron pole-pieces at the ends of the conical coils.*]

876. LONTIN, 3,264. "Improvements in Dynamo-Electric and Magneto-Electric Machines."

In order to avoid the heating of the various parts of a dynamo-electric machine, he multiplies the parts, so as to diminish the frequency of induction in the different coils.

Places on the rotating cylinder two sets of flat radial coils, their greatest dimension being parallel, and their shortest being at right angles to the axis of rotation, one set of coils being employed in producing the currents for magnetising the stationary electro-magnets, the other to produce the current used externally.

Proposes to place a number of bobbins radially on an

iron cylinder, the bobbins arranged in a *screw form*; one set of bobbins on the cylinder being used to magnetise the inducing magnets, the other set to produce continuous or reverse currents. In this machine the two inducing magnets are fixed tangentially, and those in which the current is produced rotate.

Proposes "to diminish the rapidity with which each coil is magnetised and demagnetised, in order to diminish the spark emitted at the collectors." This is done by tailing off the end of the armatures of the stationary inducing magnets.

Explains his method of connecting up the bobbins.

Patents the right of producing a current, sending it through a voltameter, and the whole or part through the coils of the inducing magnet.

Suggests the use of a dynamo-electric machine as a break.

Proposes modification of the machine in which the rotatory shaft becomes the inductor.

Proposes to use two machines, one to produce a constant current to magnetise the magnets in the other.

Proposes the following machine:—

An electro-magnetic cylinder has at its ends two wheels of soft iron, the naves being an enlargement of the axes of the wheels. The spokes of each wheel are without wire, and turn between two tangential electro-magnets.

To avoid irregularities produced by the shortening of the contact-springs produced by wear, he places the rubber vertically, fixing it by a set screw to a horizontal lever pressed down by a spring.

Suggests as a collector a toothed wheel, each tooth being insulated from the next, and geared into two metallic tooth wheels, which carry off the electricity.

1876. WESTON—HADDON, 4,280. "Improvements in Magneto-Electric Machines."

A number of radial electro-magnets arranged like the spokes of a wheel, but narrow in the plane of the wheel

and wide at right angles to the plane, rotate within a number of similarly shaped magnets arranged radially inside an iron hoop, which is itself either coated or not coated with insulated wire. A constant current is passed through the outer coils, and a current is induced in the inner rotatory coils.

All the current generated is passed through the inducing magnets.

When the machine is used for electroplating, it is necessary to prevent the current produced by polarisation reversing the polarity in the magnets as the machine slows. This he does by an automatic arrangement depending on the curvature produced in a surface of mercury when rotating. When the machine is going slowly, the surface of the mercury is approximately level, and the magneto-electric machine is short circuited; but when the machine is rotating rapidly, the surface of the mercury is depressed, connection is broken, and the current generated passes through the external circuit.

1878. Gramme and D'Ivernois—H. Johnson, 953. "Improvements in Electro-Magnetic Machines."

Electro-magnets like the spokes of a wheel, with expanding tangential pole-pieces, rotate inside a ring of iron wire wound with many bobbins quite distinct from one another, and in which separate alternate currents may be produced. In the form shown in figure 7, all the coils *a* are joined together, all the coils *b*, all the coils *c*, and all the coils *d*, in which four internal circuits four distinct currents are induced, to be used for four distinct external circuits. The revolving inducing bobbins *B*, as well as the stationary ones *a*, *b*, *c*, *d*, and the iron ring on which they are wound, are flat, the greatest dimension being parallel to the axis of rotation, or at right angles to the plane of the figure.

1878. Wilde, 1,228. "Improvements in the Construction and Working of Electric Telegraphs."

Sends his primary current, produced by a magneto-

electric machine, through a number of distinct induction coils, from any one of which a current may be obtained for signals or other purposes.

Revolves a ring similar to that described in the previous patent, No. 953, of Gramme's, inside a number of electro-magnets with their axes parallel to the axis of rotation. The currents are produced in the revolving ring: some of them are, by means of a commutator, made constant in direction for magnetising the exterior electro-magnets, and the remainder produce reverse currents for external purposes.

As an alternative, suggests that the ring may be stationary, the electro-magnets radial and rotating inside.

[*The latter portion of this patent appears to closely resemble in principle No.* 953 *of Gramme and d'Ivernois.*]

IX.

BOBBINS AND MAGNETS *FIXED*—ARMATURE *MOVING*.

[*The principle of this form of machine is the same as that employed by Sir C. Wheatstone, in the magneto-electric apparatus attached to his A B C instrument.*]

1861. Wilde, 1,994. "Improvements in Electro-Magnetic Telegraphs."

In place of a horse-shoe magnet, uses a compound bar, *a a*, figure 8, bent into a circle. Attached to the two ends are soft iron cores, *c c*, wound with wire, and a rectangular iron armature, *d*, of which the length in the position shown in the figure is perpendicular to the plane of the paper, rotates in front.

Or a star-shaped magnet has its arms, which are adjacent, magnetised oppositely. These are armed with coils, and a similar spoke-shaped soft iron armature on the same axis revolves in front in a parallel plane.

1861. Wilde, 2,997. "Improvements in Magneto-Electric Telegraphs, and in Apparatus connected therewith."

Bobbins with iron cores are fixed on the ends of

permanent compound horse-shoe magnets, arranged radially round a circle, with their ends pointing to the centre. Radial armatures of soft iron rotate inside.

Describes a number of ways of arranging the bobbins.

1873. WILDE, 618. "Improvements in Machinery and Apparatus for Producing, Regulating, and Directing Electric Light."

This is an improvement on No. 842, 1867, given in Section No. VI. Instead of a circle or circles of coils revolving between two circles of electro-magnets, the axes of all being arranged parallel to the axis of rotation, the helices are coiled round the extremities of the iron cores of the electro-magnets, which are prolonged for the purpose, and an iron disc (the periphery of which is divided into as many segments as there are electro-magnets in each circle) is made to revolve between the circles of stationary electro-magnets.

1877. VARLEY, CROMWELL, 270. "Improvements in Apparatus for Generating Electricity."

The coil in which the current is produced is fixed on a stationary permanent or electro-magnet. Various plans are described by which a single armature may have given to it a reciprocating motion. Suggests putting the armature on a tuning-fork.

[*Using the vibrations of a tuning-fork for producing motion in a magneto-electric machine has been recently patented by Mr. Edison.*]

A number of armatures rotating on a wheel of gun-metal is also described.

Proposes to combine his electrostatic multiplier with the magneto-electric machine for producing the electric light, so that the discharge from the multiplier may re-start the light, should it go out.

1877. JABLOCHKOFF, 3,187. "A Magneto-Dynamo-Electric Machine."

A flat electro-magnet has an iron core and ends, which revolve while the wire is stationary. The iron ends are

made in the form of toothed wheels. Two horse-shoe electro-magnets have pole-pieces as large as the thickness of the coil, and these pole-pieces are arranged parallel to the axis of rotation. The teeth of the two ends of the electro-magnet are at such distances apart that, when two teeth of one of the ends are opposite the two north poles of the electro-magnets, two of the other end are opposite the two south. On rotating the core through an angle equal to the angle between two teeth, the two teeth of the end that were opposite the north poles are now opposite the south, and the two teeth of the other end that were opposite the south poles are now turned away, and two others brought opposite the north.

X.

BOBBINS SOMEWHAT OF THE "GRAMME" TYPE, BUT NOT WITH THE "GRAMME" PRINCIPLE.

1852. ROBERTS, 14,198. A ring composed of permanent magnets, each of the form of an arc of a circle and separated by non-magnetic matter, is surrounded by helices of wire, not touching it. One rotates relatively to the other.

A A and Z Z, Figure 9, are the magnets; *C C*, the non-magnetic matter separating their poles. The magnetic ring is revolved by a cord passing over a groove round its edge. *B B B* are friction wheels on which the magnetic ring runs.

1872. SIEMENS, 1,919. "Obtaining and Applying Magneto-Electric Currents."

Proposes to place one magnetic pole inside a magnetic ring of the opposite polarity, and to move wire backwards and forwards in the intervening space, thus producing alternate currents.

1877. RAPIEFF, 4,432. "Improvements in the Production and Application of Electric Currents for Lighting," etc.

Makes a magnet out of an iron ring like a Gramme *ring*, but the wire, instead of being continuously coiled, is

coiled so as to make a succession of poles in the ring. Or he makes his magnet out of a number of wires, each separately coiled, or the revolving bobbin out of a number of wires separately coiled to avoid heating.

Suggests the combination of magneto-electric induction and current induction.

The whole is a very long patent, illustrated with eighty diagrams of bobbins and various parts of magneto-electric machines.

XI.

BOBBINS ON THE "GRAMME" PRINCIPLE.

[*The characteristic feature of this class of machine is that the wire forming the bobbins constitutes a circuit which is always closed, independently of the external circuit, just as is the circuit formed by two rows of galvanic cells joined up in parallel circuit, the cells in each of the rows being of an equal number, and in series. No current, however, is in either case produced until the external circuit is closed.*]

1870. GRAMME AND D'IVERNOIS, 1,668. "Improvements in Magneto-Electric Machines."

Produces a continuous current without the old form of commutator. Revolves either the bobbin or the permanent or electro-magnets, and produces either continuous or reverse current.

1872. GRAMME AND D'IVERNOIS, 1,254. "Improvements in Magneto-Electric Machines."

Proposes rubbers in the form of a bundle or brush, in place of springs or rollers, since a brush is less liable to become worn, and the production of sparks is avoided.

1872. SLATER, 1,628. "Improvements in Apparatus for Obtaining Electric Light and in Magneto-Electric Machines to be used therewith." *Provisional protection only.*

Uses one or more pairs of rings of helices, each ring being composed of an annular core made up of a number of concentric rings of hoop-iron lapped with insulated copper

wire, so as to form a series of distinct helices, the terminal wire of each helix being connected with one of a corresponding number of studs disposed in a circle in a piece of insulating material, which carries the aforesaid ring of helices. On diametrically opposite sides of each pair of rings of helices he places a U-shaped permanent or electro-magnet, covered with insulated wire joined up with the helices.

1873. JOHNSON—FONTAINE, 1,180. "Improvements in Magnets."

Proposes to use thin flexible blades or steel wires, united in a bundle and held together by polar pieces of soft iron or by non-magnetic ties or bands, to be used in the Gramme or other machines, or for other purposes.

1873. COURTENAY, 1,450. "An Improved Magneto-Electric Induction Machine." *Provisional protection only.*

1st. Proposes a plan by which, he says, the currents are collected at the terminals without break, leakage, or friction, and do not require a commutator. Provisional specification not clear. Says he joins up the whole of the helices at one end to a collar of copper or other conductor, the other ends being connected with a conducting rod insulated at the centre.

2nd. Suggests a box of mercury as a collector.

1873. WERNER SIEMENS, AND ALTENECK—C. W. SIEMENS, 2,006. "Improvements in Apparatus for Producing and Regulating Electric Currents, such Apparatus being particularly applicable for Electric Lighting."

Proposes the present form of Siemens' machine. Between the poles of one or more magnets or electro-magnets is fixed an iron cylinder, leaving a space between its periphery and the faces of the magnetic poles. In this annular space a cylindrical sheet of light metal, round which insulated wire is wound, is caused to rotate.

Or the wire may be wound round the inner iron cylinder, and this caused to revolve.

Modification :—

Two magnetic poles of opposite polarity hollowed out

to a cylindrical form are arranged opposite to each other, and in the centre between them is fixed a cylinder of iron wire, a wire coiled on it as in a Siemens' armature. In the annular space between this central cylinder and the cylindrical poles outside it, is mounted a cylindrical shell of iron having insulated wire wound round it longitudinally.

Insulated pieces of metal connected with the coils, and on which the springs, rollers, etc., press, are made elastic, to increase time of contact, and so diminish the spark.

1873. KILNER, 2,996. "Producing a Continuous Current of Electricity," etc. *Provisional protection only.*

Rotates inside a Gramme ring, and in the plane of the ring a permanent or electro-magnet, which forms a diameter of the ring. The ring has a soft iron core.

1876. KILNER, 802. "Improvements in Apparatus for Producing a Continuous Current of Electricity."

Repetition of Patent No. 2,996 of 1875, with additional devices for altering the electromotive force produced by the machine.

1877. SCHUCKERT—SIMON, 4,464. "Improvements in Dynamo-Electric Machines." *Provisional protection only.*

The armatures of the Gramme machine, instead of merely being tangential to the bobbin, surround the portions of the bobbin which is near the armatures.

1878. HADDAN, 2,003. "Improvements in Apparatus for the Generation and Application of Electricity for Lighting."

Uses a Gramme ring wound in sections, combined with a commutator of such a form that, when the ring is least useful, it is cut out of circuit. Between the sections of the wire the iron of the ring projects, partly in order that it may be brought very close to the poles of the inducing electro-magnets, and partly that by its surface it may radiate the heat generated in the coil. The iron composing the revolving ring consists of two distinct pieces, divided by a ring of insulating material inserted in a plane per-

pendicular to the axis of rotation, in order to avoid induced currents.

A permanent magnetic field is maintained, the current which passes round the inducing magnets being the same, whether the machine be opened or closed, provided that it is running. This result he obtains by winding the inducing magnets with fine wire, compared with that of the rest of the circuit, so that the current shunted by the fine wire is always nearly constant.

XII.

MISCELLANEOUS PATENTS OF MACHINES.

1848. Staite, 12,212. "Application of Electricity to Lighting."

Suggests using a galvanometer for the absolute measurement of the current passing through the lamp.

To make Magnets.—Best charcoal iron to be "converted," not in the ordinary manner, but only by a slight carbonization, or "just steeled through." The blistered steel then to be melted and cast, and the ingot rolled.

Hardening.—Heat the steel in a bath of molten metal, preferably of lead, first polishing the magnets to prevent the lead adhering, and take care that the heat is only just sufficient to harden the steel on taking it out and plunging it into cold water.

1851. Millward, 13,536. "Certain Improvements in Electro-Magnetic and Magneto-Electric Apparatus."

Instead of steel magnets acting as inductors, uses soft iron, magnetised by steel magnetic plates at the sides.

1865. Faucheux, 1,368. "Improvements in Rotatory Magneto-Electric Machines."

The current produced in the rotatory part is led to a collar on the revolving shaft, but insulated from it. This collar turns round in a lubricating box, to which it conducts the current. This lubricating box is insulated from the body of the machine, but has a binding screw for the attachment of an insulating wire.

1876. JABLOCHKOFF, 836. "An Improved Electro-Magnet."

Uses a thin metallic band wound on the iron core, so that one of the edges of the band is in contact with the core, the flat surfaces of the band being separated with paper.

[*This mode of winding was described by King in his patent, No.* 11,188 *of* 1846, *Section IV.*]

Describes an electrolytic mode of preparing such copper spiral strips.

1876. FULLER, 1,557. "Improvements in Magneto-Electric Machines." *Provisional protection only.*

Relates to magneto-electric machines, which employ two sets of electro-magnets and two revolving armatures, and which have a bridge or connecting plate so arranged and connected to said magnets as to constitute the neutral magnetic point in both sets. (*Specification not clear.*)

1861. PULVERMACHER, 2,655. "Improvements in Galvanic and Magneto-Electric Machines."

By means of centrifugal force he causes the magnets and the bobbins to come into absolute contact, and then to be suddenly separated.

1860. JOHNSON, 3,142. "Improvements in Magneto-Electric Machines." *Provisional protection only.*

Coils are grouped in fours.

Cores of electro-magnets are made tubular, with a slit to prevent induced currents.

To collect the currents, one set of terminals of the bobbins is connected with the centre of the revolving shaft, and the other set with the outside of the shaft, which is insulated from the centre. The terminals are, therefore, the axle-box, and a screw pressing against the centre of the axle.

1878. SPALDING, 1,878. "Improvements in the Method of Originating and Developing Electric Currents." *Provisional protection only.*

Suggests bringing into concerted or simultaneous action a series of dynamo-electric machines, by means of which a feeble current produced in the first may be

intensified by successively passing through the remaining ones.

[*This idea was described by Wilde in his patent,* 3,036 *of* 1863.]

. BURGIN, 3,243. "Improvements in Magneto-Electric Machines, and in Electro-Motive Engines."

Builds up the rotating cylinder of a number of elementary pieces of iron, each with wire coiled separately on it. Places the sections in the commutator obliquely to avoid irregularities in the current.

XIII.

THE "DE MOLYENS" MAGNET, BETTER KNOWN AS THE "CAMACHO" OR "ALTANDI" MAGNET.

1841. DE MOLYENS, 9,053. "Production or Development of Electricity," etc.

Proposes to place, side by side, a number of separate covered wires along a flat iron ribbon and then to roll them up so that there is iron on both sides of every portion of the wire; or introduces cylinders of iron one into the other, covering each separately with insulated wire.

1855. HJORTH, 806. "An Improved Magneto-Electric Battery."

Improvement, without change of principle, on Patent 2,198, the principal change being that of making the electro-magnets of concentric coiled tubes.

1873. CAMACHO, 3,461. "Improvements in the Construction of Electro-Magnets."

Makes a magnet of a number of concentric iron tubes, insulated wire being wound between each pair. Thinks that the current is strengthened by the induced current.

1877. H. SMITH, 3,981. "Improvements in Electric Magnets," etc.

Places an iron tube outside the magnet, the core and tube being joined together by an iron sole plate.

1877. RAPIEFF, 4,432. "Improvements in the Production and

Application of Electric Currents for Lighting and other Purposes."

Patents one of these magnets.

1877. VARLEY, 4,435. "Improvements in Electrical Apparatus for Lighting and other Purposes."

See this Patent in Section VI., Machines.

1878. JENSEN, 1,927. "Improvements in the Construction and Application of Electric Apparatus."

Winds on a bar or bundle of small central bars a certain number of coils of insulated wire, then surrounds this with a layer of iron rods arranged parallel to the central rod. On this is wound more wire, then more iron rods, etc.

SUBDIVISION NO. 2.

LAMPS.

I.

DISCOVERY OF THE ELECTRIC LIGHT, AND THE FIRST PATENT EMBRACING IT.

1813. Discovered by Sir HUMPHREY DAVY. Soon after the invention he had a voltaic battery of 2,000 cells made. He could establish the light when the points were $\frac{1}{30}$th of an inch apart, and maintain it when they were separated by 4 inches in air, or 6 to 7 inches in a vacuum.

The light, however, was unsteady from want of constancy in the battery.

[*Prof. Sylvanus Thompson informs me that there is some doubt about this conventional date* 1813 *for the discovery of the electric light, and that there is good reason for thinking Sir H. Davy discovered it a few years earlier.*]

1840. PINKUS, 8,644. "Improvements for supplying Motive Power."

States that the electric current used to produce heat to soften the cocoa-nut oil and unseal the gas valves

used in his apparatus, to work electro-magnetic couplings, breaks, etc., for engines, may be employed to produce electric glow or brush either in the air or in rarefied or condensed combustible or non-combustible gases, for the purpose of giving light; and proposes using Taylor's electro-magnetic engine for generating the current.

[*In making this classification, no other notice of this "electro-magnetic engine" has been observed in the specifications of electric patents.*]

II.

INCANDESCENCE.

(*As in the Edison Lamp.*)

1841. De Moleyns, 9,053. "Production or Development of Electricity, and its Application for the attainment of Illumination and Motion."

Uses a coil of platinum wire at the base of which is a piece of spongy platinum, and into which falls a shower of finely pulverized boxwood charcoal or plumbago, the whole being enclosed in an exhausted globe.

The coil of platinum wire is made in two parts, *E E*, figure 10, the ends of which may be brought into contact or not by means of the copper rods, *D D*, sliding in the stuffing boxes, *C C*. *D* is the tube with a conical end holding pulverized charcoal.

1845. King, 10,919. "Electric Light."

Application of continuous metallic and carbon conductors intensely heated by the passage of a suitably regulated current of electricity. Uses Toricellian vacuum when carbon is employed.

1848. Staite, 12,212. "Improvements in the Construction of Galvanic Batteries, in the Formation of Magnets, and in the Application of Electricity and Magnetism for the purpose of Lighting," etc.

Uses an iridium or an iridium and platinum wire, and

envelops the holders in glass or other non-conducting material to prevent loss of heat and light.

[*The use of a compound wire of iridium and platinum has recently been proposed by Mr. Edison.*]

1850. SHEPARD, 13,302. "Electro-Magnetic Apparatus for obtaining Motive Power, Light, and Heat."

In a ground-glass globe, or one obscured in the inside, exhausted, a vertical rod of carbon is by means of a leaden weight pushed down into a small carbon cone constituting the terminal.

1852. ROBERTS, 14,198. "Improvements in the Production of Electric Currents in obtaining Light," etc.

Patents complete apparatus for rendering a rod of graphite, coke, or charcoal incandescent in a non-combustible atmosphere.

1872. KONN, 3,809. "Improvements in the Means and Appliances for the Production and Maintaining of Light by Electricity."

Renders graphite, in an atmosphere of nitrogen, incandescent by an electric current.

1875. KOSTOFF, 441. "Improvements in the Production of Electric Light and Apparatus therefor."

To ensure better contact between the leading wires and the carbon, he introduces them into holes in the carbon.

To avoid the preparation of nitrogen in which to render the carbon rod incandescent, the air is rarefied by the heat of the carbon and expelled through a valve, the remainder being turned into carbonic oxide.

1875. KONN, 970. "Improvements in the Construction of Electric Light Apparatus."

A number of carbon rods of different lengths are supported in a proper stand in a vacuum, so that any one can be rendered incandescent. A weighted contact-maker falls into contact with the end of one rod, which then becomes incandescent. If this rod breaks, the contact-maker falls into contact with the next rod, and so on.

1875. KOSTOFF, 2,767. Appears to be identical with the complete

specification No. 441, but it says the later one, No. 2,767, only received provisional protection.

1876. FACIO—BREWER, 3,462. "New or Improved Method of, and Apparatus for Obtaining Light by Electricity." *Provisional protection only.*

Adapting Geissler's tubes to watches, etc.

1878. REYNIER—THOMPSON, 2,399. "Improvements in and appertaining to the Obtaining of Light by means of Electricity, etc."

A thin vertical carbon rod presses on a carbon wheel, of which the plane is vertical. The former descends as it is burnt, and the latter is turned either directly by the tangential force of the rod or by other mechanism.

1878. WERDERMANN, 2,477. "Improvements in Apparatus for Electric Lighting."

A thin carbon rod which is the positive pole is pushed up against a carbon plate which is the negative pole. When a number of lights are joined up in parallel arc, a switch to each is provided, such that, when one light is intentionally extinguished, a resistance equal to that of the light is inserted in its place, and the light in the rest of the lamps is unaltered.

III.

FLOWING ELECTRODES.

1853. BINKS, 119. "Improvements in producing Electric Light." *Provisional protection only.*

Two separate reservoirs of mercury, one connected with the positive, and one with the negative pole of the battery. The mercury from one reservoir is made to drop into the other, or a thin steam to flow from one reservoir into the other; or drops of mercury made to fall on to charcoal which is attached to the other pole. The mercury evaporated is collected and condensed, the whole lamp being in a closed vessel.

1856. WAY, 2,547. "Improvements in Obtaining Light by Electricity."

Mercury in connection with one pole of the battery flows through an orifice on to a point, the distance between which and the orifice can be adjusted.

1857. HARRISON, 1,412. "Improvements in obtaining Light by Electricity."

1. Proposes that the current should decompose or vaporise compounds of metal with other elementary bodies.

2. The manufacture and employment of electrodes of melted metal, and of compounds of two or more metals when made fluid by heat.

Proposes to use two pencil carbon electrodes in combination with one positive stream electrode, and to arrange these in a right line, one on each side of the stream, and having their motion controlled by an electro-magnet and keeper or by a helix.

1857. WAY, 1,258. "Improvements in obtaining Light by Electricity," etc.

Two flowing meeting streams of mercury, the circuit being rapidly made and broken by an electro-magnetic revolving commutator, in order to economise power, since the same amount of light is thus given with less consumption of material.

1857. WAY, 2,841. "Improvements in Obtaining Light by Electricity."

1. Two horizontal carbon rods are pressed together by means of springs, etc., the points of junction of their ends being fixed at the breaking point of a stream of mercury or other flowing electrode.

2. A screw motion given to a revolving carbon cylinder forming one pole, while drops of a flowing electrode falling on it form the other.

3. A carbon is pushed through a nozzle as its end is burnt away by a spring, like a Palmer's candle. On to it falls a flowing electrode.

1858. HARRISON, 1,099. "Improvements in Obtaining Light by Electricity."

A fine stream of mercury flows through a tube into a

reservoir of mercury, the light being produced at the surface of the reservoir, which is made of lamp black and silica. In the supply tube there is a tap worked by electro-magnetic action. To maintain a constant distance between the surface of the reservoir and the point where the stream breaks, the lower reservoir is placed on a float in the upper reservoir, from which the stream falls, so that the reservoir is raised as the depth of the mercury diminishes in the upper one.

Or the waste condensed mercury may be made to elevate the lower reservoir.

To prevent condensation of mercury on the glass of the lamp, a stream of water or other liquid flows over the inside, or the lamp is wholly or partially filled with a liquid which is caused to flow through the lamp.

1875. PROSSER, 3,466. "Improvements in Lamps Adapted to the Electric Light and in the Means employed for the Supply of the Electric Fluid thereto."

A stream of mercury is discharged from an indestructible nipple formed of calcareous or other suitable material, and falls on the negative carbon.

To prevent the escape of the mercury vapour, the whole is enlosed in a glass case, and to prevent the cracking of the glass by the inequalities of temperature, a second glass surrounds the first at half an inch distance from it. May use several streams of mercury. The lower or negative carbon may press against a stop of iridium, to keep it at a constant distance from the stream.

1877. RAPIEFF, 4,432. "Improvements in the Production and Application of Electric Currents for Lighting."

Proposes to feed the electric lights with flowing substances—some hard liquid or gaseous matter.

1878. SCOTT, 861. "Improvements in Apparatus for the Production of Light by Electricity." *Provisional protection only.*

To avoid waste of carbon he delivers a stream of finely divided carbon upon the ignited carbons.

Causes two of his flexible carbon bands (*vide Manufacture* of Carbons) to pass over two rollers slightly inclined

to one another, the current passing from one band to the other forming the arc.

Suggests, as a modification of this, two rotating carbon rollers slightly inclined to one another.

[*Although there are nine patents for employing flowing electrodes, no such system is, as far as I am aware, now in practical use.*]

IV.

OSCILLATING ELECTRODES.

(*As in the Houston and Thomson Light.*)

1853. BINKS, 119. "Improvements in Producing Electric Light." *Provisional protection only.*

One carbon made to approach and recede (but without actual contact) rapidly.

May be produced by the vibration of wires or by the rotation in opposite directions of two discs carrying carbon radial arms crossing one another scissor-wise; or by the rapid revolution of two carbon discs with serrated edges.

Or in the vibration actual contact may be completed each time.

1857. HARRISON, 588. "Improvements in Obtaining Light by Electricity."

Electro-magnets employed to produce vibratory movement, and mercury employed as the positive electrode.

There are two negative electrodes so arranged that, when one is in contact with the positive electrode, the other is separated, the motion being produced by electro-magnets. Proposes to use steel armatures in place of soft iron for these electro-magnets, so that the residual magnetism shall serve to retain, until the arrival of the next current, the electrodes in the position last assumed.

Proposes to produce light by a secondary current, the battery current being used as the primary current, and reversed, not broken. He says the primary current may still regulate the motion of the electrodes.

[*This appears to be one of the earliest patents to use a secondary coil in producing the electric light.*]

1867. SIEMENS, 261. "Improved Methods for Developing Powerful Electric Currents," etc.

To overcome difficulty of retardation in cables used for conveying the electric current from the land to light a beacon, he proposes to use the spark on breaking contact as his source of light, an arrangement like a trembling bell being placed on the beacon to break contact. Proposes to break contact by a point dipping into mercury.

1869. FLEEMING JENKIN, 390. "Improvements in Apparatus for Producing Electric Light."

A condenser on a beacon is rapidly charged and discharged from a battery on the shore by means of an oscillating contact-maker, the sparks produced on charging and discharging constituting the light. The beacon and the shore are joined by a cable. No induction coil is used.

1874. DE MERSANNE—JOHNSON, 1,446. "Improvements in the Production of Electric Light and in the Apparatus employed therein."

Four rotating discs composed of carbon or of other suitable material are employed, arranged two by two opposite each other, and actuated by cams or eccentrics, which impart to the discs an alternate rising and falling motion, so as to cause sparks to be produced between each pair of discs, one after the other. Rotatory motion is imparted to the discs, which are also brought together mechanically as they wear away.

Suggests a number of contrivances for subdividing the current, forms of metallic brushes to avoid heating at the collectors, etc.

V.

CARBON DISC OR DISCS.

(As in the Reynier Lamp.)

1845. WRIGHT, 10,548. "Certain Improvements in Apparatus for the Production and Diffusion of Light."

Proposes revolving discs of carbon or other material,

the current to pass from one to the other. Shown in figure 11.

1848. STAITE, 12,212. "Improvements in the Construction of Galvanic Batteries, in the Formation of Magnets, and in the Application of Electricity and Magnetism for the Purpose of Lighting," etc.

Substitutes slowly rotating carbon disc for upper rod, and adds a scraper to clean it in its turning.

1848. LE MOLT, 12,219. "Improvements in Lighting by Electricity."

Makes carbon discs in the same or perpendicular planes revolve by clockwork, and after each revolution approach one another so as to maintain a constant distance.

1876. SAMUEL VARLEY, 4,905. "Improvements in Apparatus for Producing the Electric Light."

A carbon rod rests on a disc of carbon bevelled at the edges and rotated.

Or makes segments of carbon and segments of an insulating material up into a wheel, the light being produced when the end of the rod rests on the insulating material.

Rotates the carbon disc by a simple electro-magnetic engine.

1877. REYNIER—M. CLARK, 2,982. "Improvements in Electric Lamps and in the Manufacture of the Carbons used therein."

Describes a number of ways of having two carbon discs inclined to one another. The carbons are rotated by clockwork, and drawn to one another by the action of a short electro-magnet moving one of them sideways in opposition to a spring which pulls it back.

1877. F. W. HEINKE, 4,275. "Improvements in the Method and Apparatus for Obtaining or Producing Electric Light." *Provisional protection only.*

Uses carbon discs or rings. Encloses them in a glass or metal case with two openings, one at the top and one at the bottom. The current of air produced by the heat of

the light turns a fan, and gives motion to the carbons so as to supply them as they are burnt.

Other methods proposed of utilizing the draught for regulating the light; one to have a small boiler over the lamp, the steam generated in which regulates the carbon discs or rods, etc.

VI.

MULTIPLE POINTS.

1846. Greener and Staite, 11,076. "Certain Improvements in Ignition and Illumination."

Solid prisms or cylinders of carbon enclosed in air-tight vessels of glass, the carbons being previously purified, and divided on the surface into numerous acute points; or by means of rods or strips of platinum also divided into numerous acute points; or by means of hollow cylinders of carbon, with surfaces either plain or acuminated, partially inserted within, and placed in perfect contact with hollow cones of platinum, either plain or acuminated, the whole enclosed in transparent air-tight vessels. Or two rods may be opposed, so that should the light between any two points go out it may continue between the remaining points.

1852. Slater and Watson, 212. "Improvements in the application of Electricity to Illuminating Purposes."

The equable consumption of the carbons is produced by the conjunction of two or more sets of carbons in different planes, or in the same plane, so as to produce several points of illumination.

1853. Binks, 119. "Electric Light."

Makes the wasting electrode of a bundle of carbons.

VII.

CARBONS COMBINED WITH A NON-CONDUCTOR.

(*As in the Jablochkoff Candle.*)

1846. Staite, 11,449. "Certain Improvements in Lighting."

Two carbons pressed continuously by springs obliquely *against* a cylinder of compressed pipe-clay, phosphate of

lime, etc., the whole in a rarefied and deoxygenated atmosphere. A lateral motion is initially given (from outside) to one of the carbons to start the light.

F^1 and F^2, figure 12, are the carbons, and P the block of refractory earth.

Various modifications proposed with three or more carbons.

Suggests a compound cylinder, shown in figure 13, descending as its circumference becomes diminshed by the burning of the arc passing between the carbon cylinder and the platinum ring. C^1 is a copper cylinder which encloses another hollow cylinder of pipe-clay, C^2, the interior of which is slightly coned in a downward direction. D is a thick ring of platinum, which is fixed on the top of the copper and clay cylinders, C^1 C^2, and bevelled off on its inner periphery as shown. E is a copper cylinder, which encloses a hollow cylinder of carbon, which again is filled by a solid conductor of pipe clay, F^2.

49. PEARCE, 12,482. "Improvements in Apparatus for Obtaining Light by Electric Agency."

1. The lower carbon is pressed up by a weight, but is prevented from coming into contact with the upper by two horizontal lateral rods of non-conducting (or ordinary previously non-heated) carbon pressed together with a spring. See figure 14.

2. The upper one is pressed down by a spring, and is prevented from descending too rapidly by its diameter being somewhat larger than that of the opening of the hollow earthenware cone through which it is forced.

74. WERDERMANN, 1,438. "An Improved Method of Cutting Rock, etc."

Uses two carbons, g g, figure 15, parallel to one another side by side, and worked by racks and pinions to keep them the proper length. Parallel to these carbon rods, but not touching them, is a carbon tube, k, through which a stream of air is blown to make a blow-pipe blast.

Suggests placing the carbon candle between the poles

of a powerful electro-magnet, so as to repel the air from the carbons.

1876. Jablochkoff—Applegarth, 3,552. "Improveme Electric Light." *Void; specification not filed.*

Proposes the electric candle, with insulating ma between the carbons, one carbon being thicker tha other. Suggests using a portion of the current to motion clockwork to keep the ends of the candle a height if necessary.

Patents the use of coloured matter in the insulato producing a coloured light.

1876. Werdermann, 4,805. "Improvements in Electric Lig Apparatus."

Uses carbon rods which are portions of circles, upper one being pivotted, and allowed by discribing an to descend. Proposes an electric candle consisting of concentric cylinders of carbon, insulated from one ano by a substance which volatilizes in the arc. In the cen of the candle is an air passage as in an Argand burn Suggests many other forms of candles and insulating su stances of the kind required.

Proposes to always coat the carbons with one of these substances, so as to protect the carbons from the air, and so diminish the combustion of the carbons, which he says diminishes the light.

1877. Jablochkoff, 494. "Improvements in Electric Lamps, and in Arrangement connected therewith for Dividing and Distributing the Electric Light.

Complete specification of Patent No. 3,552 of 1876.

1877. Denayraise, 3,170. "An Automatic Kindler for Electric Lights."

An arrangement by means of which when one Jablochkoff candle is burnt down, the current is cut off and sent through another one near.

1877. Rapieff, 4,432. "Improvements in the Production and Application of Electric Currents for Lighting," etc.

When the distance between the carbons is a fixed one,

as in the Jablochkoff candle, suggests as a lighter a graphite, or carbon, or metallic, rod, or plate, between the carbon points, which is either consumed or removed, or repelled by the current or by the heat of the voltaic arc on lighting. When using a Jablochkoff candle, he employs the heat generated by the candle when it has burnt down to a certain point to fuse together two pieces of metal, which brings into circuit a new candle.

VIII.

APPARATUS FOR MOVING THE CARBONS, BUT WITHOUT ELECTRIC CONTROL.

[*The characteristic of this class of lamp is that a mechanical contrivance is furnished for bringing the carbons together as they are consumed; but there is no automatic arrangement by means of which the rate at which the carbons are approached is regulated by the actual rate of consumption at any particular time.*]

1846. Staite, 11,449. "Lighting by means of Electricity."

Proposes clockwork to bring carbons together, the rate of the clock being previously regulated to suit approximately the consumption of the carbons. In order that the carbons shall be initially separated by the right distance, a spring is attached to the carbon. On pressing this spring, *L L*, figure 16, with the finger, the carbons are brought in contact, and then, on liberating the spring, they are separated by the least space that there ought to be between the carbons. The whole is placed in a rarefied and deoxygenated atmosphere. The upper carbon is flat, the lower conical. *G G* are pieces of platinum, by which the current is conveyed into the lower carbon at the top, to avoid the introducing the resistance of the carbon.

1846. Pearce, 12,482. "Improvements in Apparatus for Obtaining Light by Electric Agency.

Lower carbon is moved up by clockwork, and the upper

carbon, which is a disc inclined at an angle to the vertical, is rotated by the same clockwork.

May use one, two, or three revolving discs with one rod.

Uses platinum wires with iridium ends to conduct the electricity into the lower carbon at the upper end.

Mode of joining carbons, by letting in a connecting piece.

1877. TIPPER, 4,412. "Improvements in the Construction of Electric Lamps."

Describes a number of simple systems of mechanism by which motion can be given to the two carbons.

IX.

MAXIMUM DISTANCE OF CARBONS FIXED BY AN ADJUSTABLE STOP.

(As in the Wallace-Farmer Lamp.)

[*The characteristic of this type of lamp is that when no current is passing, the carbons are brought instantly into contact by a weight or spring. When the current passes, the carbons are sharply separated by the action of an electro-magnet to a fixed distance, which is quite independent of the strength of the current.*]

1852. ROBERTS, 14,198. "Obtaining Light, Motion, etc., by the Agency of Electricity."

When the current passes, the upper carbon is clipped and raised until stopped by a screw adjusted so that the distance between the carbons shall not exceed a certain amount. When the current stops, the upper carbon falls on to the lower.

M, figure 17, is an electro-magnet, turning on a pivot at the centre of the rod *c*. When no current passes the iron carbon-holder *H*, and the carbon, which it holds, fall by their weight, until stopped by the upper carbon coming into contact with the lower. When the current passes, the electro-magnet attracts *H*, and is also itself

attracted by the piece of iron *I*. The consequence is *H* is raised until stopped by a screw previously adjusted in position. Either the face of the pole of the electro-magnet, or the surface of *H*, is coated with a non-magnetic substance to enable them to separate quickly when the current becomes weakened.

1852. SLATER AND WATSON, 212. "Improvements in the Application of Electricity to Illuminating Purposes."

1. When the current passes, the upper carbon is gripped and raised a fixed distance, by an electro-magnet.

2. The electro-magnet below grips and raises the upper carbon through a system of levers.

1855. CHAPMAN, 739. "An Improved Electro-Mechanical Apparatus for Supplying and Adjusting the Electrodes used in the Production of the Electric Light."

1. On the current passing, an electro-magnet pulls down the lower carbon a fixed distance, and another electro-magnet presses a break on a wheel round which is wound a chain holding up the upper carbon.

2. Or one electro-magnet may, by pressure of the break, prevent the upper carbon slipping, while another electro-magnet raises it a fixed amount on the commencement of the current.

An arrangement similar to that described in No. 1 is also applied to horizontal carbons, a weight is this case giving motion to one of them on the break-wheel slipping.

1857. HARRISON, 558. "Improvements in Obtaining Light by Electricity."

1. The positive pole of the battery is connected with a hollow horizontal cylinder of carbon, to which a rotatory as well as a motion of transtation parallel to the axis is imparted. The negative pole is attached to a vertical rod carbon, which by means of an electro-magnet is separated to a fixed distance on the passage of the current. When the current ceases the end of the rod falls on the rotating cylinder. The cylinder is made to rotate by clockwork started by the current.

2. The positive pole is attached to a carbon disc rotating in a vertical plane; the negative to a vertical rod, the end of which is raised from the disc a fixed distance on the passage of the current. A small roller runs on the lower side of the carbon disc, and the position of the roller determines the height to which the rod is raised. As the edge of the disc burns away, this roller rises, and the carbon rod drops the same amount.

1878. WALLACE—HUGHES, 240. "Improvements in Electric Lamps."

Two rectangular slabs of carbon are arranged with their long sides parallel to one another, and the distance adjusted so that the arc can be formed. When by the burning away of the carbons, the arc can hardly be supported at the place it is burning, it then attacks the next point, working backwards and forwards on the edge, until so much of the carbon over that portion is consumed as to nearly break the circuit; this will force the current to an adjacent point, where a less arc is formed.

1878. HADDAN, 2,003. "Improvements in Apparatus for the Generation and Application of Electricity for Lighting," etc.

A vertical electro-magnet has a movable core kept in position by a spring. When the current passes, the core is pulled up: this causes a sort of eccentric to raise and hold the rod bearing the upper carbon, which falls down again when the current is weakened. A number of plans are described for accomplishing this result.

Either electroplates his carbons with copper, or twists a copper strip round them.

X.

REGULATED MOTION OF THE CARBONS IN ONE DIRECTION ONLY.

(As in the simple form of Dubosc Lamp.)

[*The speciality of this type of lamp is that when the current is too weak to support a proper light, one or both of the carbons is*

slowly moved by clockwork until they are at a proper distance apart, when the current stops the clock by electro-magnetism. If, however, from any cause the carbons approach too near one another, then there is no automatic contrivance for separating them.]

1852. JACKSON, 14,330. "Obtaining Light and Motive Power."

When the current passes, a hollow iron tube is sucked into an electro-magnet. This stops the upward motion of the lower carbon produced by a spiral spring, but the current cannot pull the upper carbon down, as this is prevented by a rack and ratchet wheel. Shown in figure 18.

1853. BINKS, 119. "Improvements in Producing Electric Light." *Provisional protection only.*

Balances lower carbon in a liquid, so that as it burns away and becomes lighter it rises.

A carbon rod is made to progress through a carbon ring, which is attached to the non-consuming electrode, and the arc is formed between the rod and the edge of the ring.

Or one slip of carbon is made to slide parallel and near to another slip, so that the point of the side or face of the long slip that is nearest to the end of the shortest slip, and the end of the shortest slip itself, shall form the striking points. Or the moving slip is made to move parallel between two fixed slips, which together constitute the non-consuming pole of the battery.

1853. FONTAINEMOREAU, 1,806. "An Improved Mode of Regulating the Electric Light."

Horizontal carbons are both pressed together by springs, causing the revolution of a fly. The current, by passing through an electro-magnet, causes the armature to stop the fly.

By a spring arrangement, two fresh carbons are put in place of the old ones as soon as they are nearly exhausted.

1855. CHAPMAN, 739. "An Improved Electro-Mechanical Apparatus for Supplying and Adjusting the Electrodes used in the Production of the Electric Light."

By a modification of the arrangement described in 739,

under "Maximum Distance," Section No. IX. of th pilation of patents of lamps, the lower carbon is r little as the lower one descends on the slipping of the wheel through the weakening of the current.

1856. Lacassague and Thiers, 2,456. "An Improved E Lamp."

The lower electrode floats on mercury in a ciste which mcre is allowed to flow as the current becomes this being effected by the valve between the two ci which was previously closed by an electro-magne opening. The opening may either be effected by a or by an electro-magnet worked by a derived current.

1857. Harrison, 588. See entry under "Maximum Dist No. IX. of this compilation of patents.

1857. Pascal, Jean Baptiste, 1,033. "Improvements in E Lamps."

If the current producing the light is weakene armature of an electro-magnet is pulled away by a sp this completes the circuit of a secondary current, the fu of which is to decompose acidulated water, the pressu the gases produced forcing mercury from a reservoir i cylinder in which floats the lower armature.

1874. Whitehouse, 1,820. "Improvements in Producing El Light."

Clockwork or other mechanism rotates rods or cyli of carbon at a slow speed, which can be regulated at ple and as they rotate they are moved gradually forwa screws or otherwise.

Reverses current occasionally to remove the ridge groove traced out on the opposite carbons.

1878. C. W. Siemens, 3,315. "Improvements in Apparatu Electric Lighting." *Provisional protection only.*

Two tolerably massive metal tubes incline down and forwards, so that carbon rods inserted in these t would nearly meet in a point in the front part of apparatus. Friction springs in the tubes prevent the ca slipping down too easily. The carbons are driven dow

clockwork, which is arrested by the expansion of a thin strip of metal, through which the current passes, when the current becomes too strong. As the carbons are consumed there are inserted into the tubes fresh carbons with pointed ends, smeared over with a cement made of caoutchouc or of carbon powder and adhesive gum. The end of the fresh carbon drops into a hollow made at the upper end of the previously inserted carbon.

XI.

BALANCE LAMPS—REGULATED UP AND DOWN MOTION.

(*As in the Foucault Lamp.*)

[*The characteristic feature of this type of lamp is that the ends of the carbons are always kept at a fixed distance apart, by a balance of two forces, the one produced by a weight, spring, &c., the other by the action of the whole, or part, of the current producing the light. Consequently, if the current is too weak the carbons are approached, or if too strong they are withdrawn from one another.*]

1847. STAITE, 11,783. "Lighting by Electricity."

The carbons are kept at a fixed distance by clockwork, which can move the lower carbon up and down, depending on whether a crown wheel works in an upper or lower pinion on the same vertical axis. The clockwork is controlled by a movable weighted soft iron core, acted on by a hollow electro-magnet, and its speed is regulated by a centrifugal break.

The lower carbon only is moved.

1848. STAITE, 12,212. "Improvements in the Construction of Galvanic Batteries, in the formation of Magnets, and in the Application of Electricity and Magnetism for the purpose Lighting," etc.

Suggests improvements in the mechanism connecting the clockwork with the lower carbon.

Dispenses with clockwork, and uses a simple weight to

draw the carbons together, while a soft iron core, sucked into a hollow electro-magnet, tends to separate them.

Proposes ratchet arrangement to prevent the iron bar being drawn out of the coil: the weight can pull up the lower carbon raising the iron rod a certain distance, and then becoming disconnected from it by the rod coming in contact with a stop, which liberates a detent, but when the iron rod is attracted down by the current, it leaves this stop, the detent is allowed to act and the carbon pulled down.

The lower carbon only is moved.

1848. ALLMAN, 12,276. "Producing Light by Electricity."

1. A weight brings the carbons together by pushing down the upper one, and the current separates them by causing a small electro-magnet to revolve in a magnetic field produced by a horse-shoe compound permanent magnet.

2. The current heats a bar, which expands and allows a weight to separate the carbons, or the current heats a metallic thermometer, and the twist separates the carbons; or the same thing is done by pressure produced by gas, generated by the current passing through a voltameter.

[*This expansion of a metal caused by the heat generated by the current passing through it has been recently employed, with certain improvements, by C. W. Siemens in his Current Regulator, No.* 2,281 *of* 1878, *and in his Lamp, No.* 3,315 *of* 1878.]

1849. PEARCE, 12,482. "Improvements in Apparatus for Obtaining Light by Electric Agency."

A magnet something like the needle of a large galvanometer separates the carbons when the current passes; a weight brings them together again on the current ceasing. Or when the current stops, a small tongue of carbon is, by means of a spring, inserted between the carbon rods. When the current again starts, an electro-magnet removes this tongue.

1853. STAITE, 634. "Improvements in Apparatus for Producing and Applying Currents of Electricity."

1. In place of the weight described in Patent 12,212

of 1848, proposes a body that will float immersed in a fluid, and therefore trying to rise.

Instead of rack proposes a friction clip, which becomes detached when the movable core of the electro-magnet rises to a certain point. Very long carbons employed.

2. Carbons may be fixed horizontally.

1872. SLATER, 1,628. "Improvements in Apparatus for Obtaining Electric Light," etc. *Provisional protection only.*

Each electrode is connected with a separate rack, geared into the opposite insulated halves of a toothed wheel. The required motion is obtained from an electro-magnet acting upon an armature in one arm of a spring-gripping lever, which embraces loosely the axis of the said wheel. When the current is established, the magnet attracts the arm of the lever, causing such lever to grip the axis and to impart a slight rotatory motion to the toothed wheel, thereby separating the electrodes more or less according to the arc required, the extent of this being regulated by an adjustable stop screw on the arm of the lever.

1873. WILDE, 618. "Improvement in Machinery and Appartus for Producing, Regulating, and Directing Electric Light," etc.

Both carbons moved by a right and left-handed screw, either controlled by hand or by electricity.

In a modified form one electro-magnet is dispensed with, and a weight or spring produces the motion the electro-magnet could effect.

1873. WERNER SIEMENS, AND ALTENECH—C. W. SIEMENS, 2,006. "Improvements in Apparatus for Producing and Regulating Electric Currents, such Apparatus being particularly applicable for Electric Lighting."

Uses two electro-magnets, one through which the current always passes, and having a low resistance. The other is of high resistance, and acts as a shunt to the light, and is brought into operation when too little current passes through the carbons. Each of these magnets is provided with an armature which vibrates when its magnet is active,

so as to work a pawl on a ratchet wheel, the first magnet being used to separate the carbons, the other to bring them together.

1874. Day—Lake, 1,261. "Improvements in Electric Light Apparatus."

A spring moves both carbons together; when the current passes an electro-magnet stops the action of this spring, while another electro-magnet slightly depresses the lower carbon.

By another method, when the current passes through electro-magnet No. 1, the rapid action of the spring is stopped, but the carbons are brought together slowly by a clockwork, allowing the spring to act slowly. If, however, the clockwork acts too slowly, the weakening of the electro-magnet removes the clockwork gearing, and allows the spring to act rapidly. Reverses automatically, from time to time, the direction of the current, to compensate for the unequal wearing away of the carbons.

1876. De Mersanne—Johnson, 3,315. "Improvements in the Production of Electric Light for Signalling," etc.

The carbons are attached to holders, each on a long vertical screw. Either of these screws can be made to rotate, and the corresponding carbon-holder to ascend or descend, by causing a pawl by its oscillation to drive onwards a ratchet-wheel, the pawl being itself the armature of an electro-magnet. The current is directed through the electro-magnet by hand.

1877. Lontin—A. N. Clark, 2,094. "Improvements in Electric Light Apparatus."

A wire in a shunt coil is heated by the current, and by its expansion regulates the distance of the carbons. To prevent its being fused, should the light circuit be broken, a piece of fine wire is inserted in the shunt circuit, which is fused before the regulator coil is affected.

A plan is described by means of which, should the fine wire be fused, a commutator is brought into action, causing the current to continue passing through any other lamps

that may be in circuit. An arrangement of commutator is described by means of which the regulator can be altered, so as to enable one lamp alone to be used in the circuit, or the same lamp with one or two or three others.

Patents shunting the electro-magnet, traversed by the light current, with a resistance equal to the resistance of the magnet itself.

1877. MARCUS AND EGGER—A. N. CLARK, 2,934. "Improvements in Electric Lamps."

The currents passing through a coil sinks in a soft iron rod which separates the carbons by moving both, the upper one less than the lower one. The weight of the upper carbon, holder brings them together as the current becomes weaker. As the carbons are separated, the same current produces less and less force on the iron core, in consequence of more and more of the upper portions of the coil being cut out of circuit by a sliding contact-maker in descending.

[*No provision seems to have been made for the effect this would produce as the carbons were burnt away.*]

1877. RAPIEFF, 4,432. "Improvements in the Production and Application of Electric Currents for Lighting," etc.

Three or four long thin carbon rods, or two thin carbon rods pressing down on a disc, have their ends brought into contact by weights, and are separated by the current passing through an electro-magnet.

To reduce the resistance he leads in the current near the burning ends of the carbon. Also coats the rods with copper.

Suggests a combination of magneto-electric machines, batteries, condensers, etc. Suggests a supply of carbons in a box, from which the light is fed by means of the top of the box being pressed down by a spring, and friction wheels carrying on the carbons as they come out of the bottom.

1878. KIPLING, 717. "An Improved Electric Lamp."

Consists of four carbons, two meeting at their points at one level, and two others also meeting at their points at a somewhat higher level, and crossing the lower pair at right

angles. The frames holding these two sets of carbons are pressed together by a spring, so as to bring all the four points into contact, which are kept separated to the required distance by the action of an electro-magnet through which the lighting current passes.

1878. DUBOS, 2,401. "Improvements in Apparatus for Producing Electric Light." *Provisional protection only.*

Patents balancing two forces, one produced by a spring weight, etc., and the other by electro-magnetism, for keeping the carbons in their proper position.

[*This is practically patenting an electric lamp, such as, for example, Staite patented in* 1847, *thirty-one years before.*]

XII.

SUBDIVISION OF THE LIGHT.

(*As in the Jablochkoff System.*)

1870. D'IVERNOIS, 1,917. "An Improved Means of Producing the Electric Light." *Provisional protection only.*

Uses several sets of carbons so as to divide the arc, and to make a number of arcs crossing one another.

1877. JABLOCHKOFF, 1,996. "A New Process of Producing and Dividing Electric Light, and Apparatus therefor."

Produces the light by means of an induction current, one induction coil being used for each lamp, so that the failure of any lamp does not stop the primary current.

[*The use of an induction current for an electric lamp was patented by Harrison, No.* 588, 1857, *Section IV., Lamps.*]

1877. JABLOCHKOFF, 3,839. "A New System of Distributing and Increasing with Atmospheric Electricity Currents proceeding from a Single Source of Electricity, for the Purpose of Supplying several Lighting Centres."

Proposes to place in the circuit of a machine giving

alternate currents and the light, a condenser, the charges and discharges of which would produce the light.

[*It is not clear what the atmosphere has to do with the method.*]

1878. HARRISON, 3,470. "Improvements in Obtaining Light by Electricity," etc.

Produces the light at each burner by means of an induction coil, the primary coil of which is in connection with a magneto-electric machine giving reverse currents.

1878. WELCH, 4,278. "Improvements in Apparatus for Dividing and Distributing or for Collecting Electric Currents," etc.

Arranges a number of movable contact-pieces around a central axis of revolution, and arranges a greater or lesser number of fixed contact-pieces around the same axis, so that a number of electrical contacts are made with each fixed piece at each revolution of the movable pieces.

XIII.

LONG CARBONS EMPLOYED.

1848. STAITE, 12,212. Carbon-holders made in the form of long tubes.

XIV.

MANUFACTURE OF CARBONS.

1846. GREENER AND STAITE, 11,076. "Certain Improvements in Ignition and Illumination."

Uses lamp black, charcoal, or coke, already purified from sulphur and metallic mixtures by the application of electricity in accordance with the process patented by Jabez Church in 1845; digests in nitro-muriatic acid; washes several times in water or in some weak alkaline solution or carburetted alkaline solution, finally with distilled water; then dries and presses with hydraulic screw, or fly press, into cylinders, and when necessary exposes to intense heat in a furnace for 24 hours.

1846. STAITE, 11,449. Takes equal quantities of coal of a medium quality (neither too rich nor too poor) and of that purified description of coke known as "Church's Patent Coke;" powders and compresses in close sheet-iron moulds until solid, then plunges into concentrated solution of sugar, and when sufficiently dry subjects it for several hours in a close vessel containing charcoal at an intense white heat.

1847. STAITE, 11,783. In addition to pressing, also heat, and when hot plunge into sugar melted by heat without the aid of any liquid. Then cool and place in a closed vessel containing pieces of charcoal heated to a white heat, and keep there for many hours or even two or three days; or the whole mass may be a second time immersed in melted sugar, and the process repeated.

He says, also, that electrodes made of gas-retort carbon frequently contain iron, which makes them split and not give out so much light. They may be much improved by heating to a white heat in a closed vessel for some days.

1848. STAITE, 12,212. Perfers to use, 1st, plumbago powder having iron, &c., extracted by washing and warming in acids; 2nd, lamp black; 3rd, charcoal powder; 4th, powder of carbonaceous concrete which is deposited in gas retorts; or 5th, sifted grains of this material. Mix any one of these with brown sugar, melt and boil (without water) until stiff, press when hot in iron moulds lined inside with paper, chalk, or plaster of Paris, to prevent adherence and to allow for escape of the gas, the moulds having holes for the same purpose. Heat slowly until a red heat is obtained, at which temperature keep them for some time, then take out and put in upright crucibles with lute; gently raise to a white heat, at which temperature keep them for some time, and then allow them to cool.

1848. LE MOLT, 12,219. "Constructing Electric or Galvanic Piles for obtaining Electric Light."

Take one part of coal, coke, or charcoal, three parts of carbon obtained from gas retorts, ground fine, and one of

tar; mould and press, dry in the shade, heat gradually in a nearly closed retort until brought to a red heat, at which temperature keep the retort for 36 hours, when cool slowly.

Makes the carbon discs used by him in his electric lamp from gas-retort carbon, cut into the right shape, and purified by solution in a mixture of nitric and muriatic acids for 12 hours, afterwards in fluoric acid for 12 hours.

1852. ROBERTS, 14,198. "Improvements in the production of Electric Currents in obtaining Light," etc.

Mixes five per cent. of lime with materials of electrodes, to increase the brilliancy of the light.

1852. JACKSON, 14,330. "Improvements in producing Artificial Light," etc.

Hollows out top of lower carbon and introduces mercury or platinum into the recess.

1853. BINKS, 119. "Improvements in producing Electric Light." *Provisional protection only.*

Subjects lignite to destructive distillation in closed vessels.

Covers metal with tar, pitch, bitumen, asphalt, and rosin, or mixtures of finely pulverized charcoal or lamp black with some adhesive material, which on being dried or strongly heated leaves a residue of solid or compact carbon.

Or drills holes in carbon and inserts metal rods, or attaches a veneering of charcoal to metal.

1853. STAITE, 634. Boils carbon in oil or other fatty substance, and bakes.

1857. HARRISON, 588. "Improvements in Obtaining Light by Electricity."

Pieces of metal or other material are placed in gas retorts, or in tubes connected therewith, for the purpose of receiving a deposit of gas carbon. Or a combination of metal powder and plumbago, or other form of carbon, may be formed into electrodes by compression. Proposes to insert other substances in the powder in order to colour the light.

1858. HUNT, 282. "Improvements in Means for Producing the Electric Light."

The residuum from the distillation of tar or pitch is reduced to an impalpable powder, and mixed with tar or other hydro-carbon; the electrodes are then moulded, heated red hot, immersed in tar, and again heated, and so on until the required density is obtained.

1877. REYNIER—M. CLARKE. "Improvements in Electric Lamps and in the Manufacture of the Electrodes used therein."

Uses powdered sugar 20 parts, cast or wrought iron filings 5 parts; the mixture is moistened with sufficient water, acidulated with from 1 to 5 per cent. of nitric or hydrochloric acid, to form a stiff paste, which is then placed in a mould made of platinised or gilt metal and compressed. The material is then dried either cold or by a very gentle heat, until it can be removed from the mould. It is then put in a stove for 24 hours at a temperature gradually raised from 40 deg. to 100 deg., after which it is maintained at a bright red heat for an hour, whilst protected from the air.

1878. REYNIER—M. GRAY, 471. "Improvements in the Manufacture of Carbon Electrodes." *Provisional protection only.*

Coats carbons with a refractory metal, as nickel or iron, to prevent undue waste of the carbons.

1878. SCOTT, 861. "Improvements in Apparatus for the Production of Light by Electricity." *Provisional protection only.*

Incorporates finely powdered gas-retort carbon or well-burned coke, or other carbon possessing good electrical conducting power, with thin flour or thin starch paste, so as to produce a plastic mass. From this, discs or blocks are made by pressure, rods by drawing.

Makes carbon ribbons with cords of asbestos or fibres of hemp, etc.

1878. HARRISON, 3,470. "Improvements in Obtaining Light by Electricity," etc.

To produce a perfectly white light he impregnates the

carbons with various metals or elementary bodies. By using threads of silk, cotton, etc., and coating them with carbon, etc., he forms a flexible electrode.

XV.

MISCELLANEOUS PATENTS.

1852. ROBERTS, 14,198. "Improvements in the Production of Electric Currents in Obtaining Light," etc.

Two sets of electrodes are fed by one battery, so that the light given by the two is nearly constant, since, as each shunts the other, if from any cause one light is feeble from too little current passing through it, more passes through the other.

Proposes to burn a candle between the carbons producing the electric light, to improve the light by heating the air.

1852. JACKSON, 14,330. "Improvements in Producing Artificial Light," etc.

Proposes a sulphuric acid rheostat to diminish, if necessary, the strength of the current.

1852. SLATE AND WATSON, 212. "Improvements in the Application of Electricity to Illuminating Purposes," etc.

Propose a rheostat formed of iron wires imbedded in pipe-clay.

1853. STAITE, 634. "Improvements in Apparatus for Producing and Applying Current Electricity," etc.

As long as the intermission and completion of the circuit is properly kept up, only a small bell is rung, but as soon as the strength of the current diminishes or ceases, a large bell is also automatically rung.

1855. LACASSAGNE, 998. "An Electro-Magnetic Regulator for Electric Telegraphing, Lighting, and Electro-motive Purposes."

By the combination of a voltameter and an electro-magnet, the constancy of the current is automatically maintained.

1860. KROTKOFF, 1,626. "Improvements in Apparatus for Employing the Electric Light." *Provisional protection only.*

Using reflectors.

1862. MORRIS AND MONCKTON, 1,516. "Improvements in Obtaining and Applying Light and Heat by Electricity."

Use induction sparks in a vacuum tube. Propose to use a condenser with the coil.

1863. KEELING, 31. "An Improvement in Lighting Halls, Theatres, and other Buildings."

Suspends the light, and puts under it a curtain to avoid sharp shadows.

1867. JOHNSON, 1,308. "Improvements in Magneto-Electric Signal Lights."

Proposes to use lenses and reflectors with a "Duclos" electric light, worked with a "Berlioz" magneto-electric engine.

1878. C. W. SIEMENS, 2,281. "Improved Means and Apparatus for Distributing and Regulating Electric Currents to Work Lamps and other Electric Apparatus."

Describes his current regulator recently shown to the Royal and Physical Societies.

1877. PROSSER—MOORE, 2,585. "Improvements in the Construction and Arrangement of Lamps adapted to the Electric Light."

Describes a number of ways of arranging carbon rods, carbon discs, fluid electrodes of mercury, etc., etc.

1878. DE CASTRO—LARE, 2,943. "Improvements in Lighting and Illuminating Apparatus."

Patents various arrangements for reflecting and scattering the electric light, for lighting different rooms with the same source of light, etc.

PATENTS FOR IMPOSSIBLE RESULTS.

1843. BARRATT, 9,786. "Gilding, Plating, and Coating Metallic Surfaces."

Describes an electrical magnetic battery in which a

current, he thinks, is produced *without motion*, simply by connecting the opposite poles of certain magnets by iron wires.

1845. PALTRINEU, 10,793. "Obtaining and Applying Motive Power."

In electro-magnetic engines thinks both bodies attracted or repelled should move, since he considers there will be less loss of work.

1848. STAITE, 12,212. "Application of Lighting to Electricity."

Fancied he increased strength of current by including in the circuit a long coil of insulated copper ribbon wound in an iron case.

1874. WERDERMANN, 3,156. "Improvements in Magneto-Electric Machine."

A horizontal iron ring, on which a continuous wire is wound, with its two ends led to the external circuit, is rotated between the poles of a number of horse-shoe magnets in such a way that the lines of force are parallel to the axis of rotation. A continuous current, the patentee says, is produced, and he therefore calls it "isopolar induction."

1876. MONCKTON, 4,597. "Improvements in Generating and Applying the Electric Current," &c.

Proposes to coil a continuous wire round an iron cylindrical tube, and to rotate the cylinder about its axis in a magnetic field.

Colonel BOLTON: My object in getting out these details was, as I stated in my introduction, to assist members of the Society in their future development of the Electric Light. What has been done in that direction in the past has, I venture to say, been comprehensively set forth in the paper of this evening. The present state of the Electric Light was recently exemplified at the Albert Hall by our Vice-President, Mr. Preece, with the admirable and exhaustive display of the various types of Electric Light which were then exhibited. The future of the Electric Light, I hope, remains with the members of the Society of Telegraph Engineers. *There is much* still to be done before the Electric

Light arrives at the perfection expected from it, since that it is the light of the future no scientific man can doubt. You will have seen that no real discovery of any importance has been made since the introduction of the Gramme machine, and that for many years past the Electric Light has stood in much the same position as it now does. The future, therefore, of this important development of electrical science is, I repeat, in your hands, and it will be for you, the Society of Telegraph Engineers, to achieve that perfection which there is every reason to believe will make the Electric Light not only a scientific, but a great commercial success.

I have to thank Professor Ayrton for the great assistance he has afforded me, not only in the preparation of this paper, but for the admirable way in which he has acted as my representative this evening; and I have also to thank you, Sir (the President), and this most attentive and patient audience for the cordial reception which has been accorded to my paper.

Major WEBBER: In proposing that a vote of thanks of the Society be given to Colonel Bolton for the paper which we have just heard read, we can only now express our regret at his inability to deliver it himself, and congratulate ourselves at the same time that Professor Ayrton, who has studied the subject a good deal, and is therefore well competent for the task, has performed his part so well. I think the impression that will be left on some of our minds is, that those who have gained by what we have heard have been not only the inventors who have during the past few years brought out so many doubtful novelties, but also that the Patent Office must have benefited very materially. This paper may be looked upon as an epoch, so far as this Society is concerned, in the question of electric lighting. It has been compiled, we must be aware, with vast labour, and it will be of great assistance to electricians in giving them a stage from which to start in advancing upon the combined experiments of the past. While there is great honour due to many inventors, we must not forget that a great number have proposed things of a very complicated nature, and have not worked them out. I think Edison is *differently* placed in this respect, and he has worked with different

ends and different results from many of his predecessors, because we are aware he is an investigator and worker, as well as an inventor. But there is one point which has not been mentioned in the paper, and it being a statement of facts it was not necessary, but it is now becoming a very important question for the consideration of telegraph engineers. It is also a question which has not been much mentioned in the discussions in other places, and we have heard very little about it in the papers which have taken up the subject so warmly—that is the laying down and construction of the lines which shall carry the current to the light. This is a good practical subject for the consideration of telegraph engineers; and I cannot help thinking that the man who succeeds in introducing a thoroughly good, practical, dielectric will do more for the spread of the electric light for general purposes than those who have been the inventors of the wonderful machines which have been brought before us to night. I will say no more, as this is a paper which does not call for discussion, than, therefore, to propose a cordial vote of thanks to Colonel Bolton and to Professor Ayrton.

Mr. Stroh seconded it.

The President: I am sure it is with the greatest pleasure I put this proposition to the meeting, because the paper displays an amount of patient and laborious research which must be of the greatest benefit to those who look forward to work in this field.

The thanks of the meeting were then accorded by acclamation.

The President, in terms of deep regret, announced the death, in India, at the early age of 32 years, from cholera, of a well-known member of the Indian Government Telegraph Department, and of that Society, Mr. R. S. Brough. He announced that Mr. L. Schwendler had been so good as to offer to prepare for the Society's Journal a memoir of the deceased gentleman.

The following candidates were balloted for and declared to be duly elected:—

As Member: Professor Herbert McLeod.

As Associates: G. H. Allden, William McIntyre, E. B. Stringer.

The meeting then adjourned.

ORIGINAL COMMUNICATION.

ON THE ELECTRO-MOTIVE FORCE OF MERCURY ALLOYS.

By C. Hockin and H. A. Taylor.

The experiments described in this paper were suggested by the curious but well known facts that the amalgamation of a zinc plate generally renders the zinc more electro-positive than it was before amalgamation, while at the same time it protects the plate to a great extent from the action of dilute sulphuric acid.

Three explanations of these facts have been given, neither of which appears to comprehend all the phenomena.

We do not profess to have succeeded in obtaining a complete explanation, but we think that some light on the subject may have been obtained by the observations we record.

Du Moncel, on the authority of Regnault, states that a metal is rendered more electro-positive by amalgamation when the amalgamation is attended by loss of sensible heat, more electro-negative when attended by the development of sensible heat.

A second statement is, that the amalgamation of a sheet of zinc mechanically covers the small pieces of foreign metal that may exist on its surface, and so prevents the formation of local currents, which tend to render the zinc less effectively electro-positive, and at the same time hasten the destruction of the metal.

A third explanation adopted by Dr. Guthrie and Mr. Jenkin is, that a plate of zinc is not commonly of uniform hardness, and that an electro-motive force exists between the softer and the harder portions. On amalgamation the mercury is assumed to reduce the whole surface of the zinc to the same mechanical condition.

To verify the facts assumed in these explanations, and to

determine the general effect of amalgamation on the electro-motive force of the metals, the following observations were made:—

1st. Certain metals, zinc, cadmium, tin, lead, iron, copper, silver, gold, platinum, were compared with amalgamated zinc in a solution of zinc sulphate, in dilute sulphuric acid (one part acid sp. gr. 1·838 to nineteen parts distilled water), and occasionally in other solutions.

2nd. The plates of metal used for the first set of observations were amalgamated, and the observations repeated.

3rd. Slight traces of the metals were introduced into pure mercury, and the electro-motive force of the mixture obtained.

4th. Solid alloys of the metals with mercury were made, and the electro-motive force of the amalgum determined.

5th. Certain alloys of the metals first mentioned were made, and their position in the scale of electro-motive force determined both before and after amalgamation.

6th. The effects of the presence of salts of mercury or of some of the more electro-negative metals in restoring the electro-motive force of mercury containing traces of the more electro-positive metals was determined, and the influence of the presence of acids that attack mercury and form nearly insoluble salts under the same circumstances was observed.

7th. The rapidity with which the surfaces of certain metals after polarization regain their normal state was observed.

The method of observation was as follows:—The current from two Le Clanché cells was passed through a total resistance of 100,000 ohms, arranged as a Varley's slide resistance.

The instrument used by us was made by Messrs. Elliott Brothers.

The electro-negative plate of the pair of metals to be compared was connected with the carbon pole of the Le Clanché cells.

The electro-positive plate of the pair was connected with one terminal of a sensitive reflecting galvanometer, the second terminal of the galvanometer with the index of the slide resistance.

The arrangement was sufficiently sensitive. An electro-motive force of 0·0003 volts was clearly indicated on the galvanometer, and corresponded to a motion of the index over one coil. The constancy of the *Le Clanché's* cells was proved by comparison from

time to time with a Clark's standard cell. The cells altered less than one per cent. in electro-motive force during four months.

By the arrangement described, the electro-motive force between two metals could be determined without allowing any sensible current to pass between them, a very important condition, as it avoids the great errors that would otherwise under most circumstances be introduced by polarization.

The method was, we believe, first used by Poggendorf.

As to the precautions taken, the metals used were commercially pure, obtained from Messrs. Johnson & Mathey or from Messrs. Hopkin & Williams.

All mercury used was redistilled after it had been employed for one comparison, and a sample of the mercury obtained after each distillation was tested before it was used to determine the electro-motive force between it and zinc.

This we found a very delicate test of the presence of any of the more electro-positive metals.

We give, first, in a tabular form a few numerical results :—

† Electro-negative Plate.	Electrolyte. Solution, 1 pt. sulphuric acid of sp. gr. 1·838 to 19 parts water. † Electro-positive plate zinc amalgamated.				Electrolyte. Solution of zinc sulphate saturated or nearly saturated. † Electro-positive plate zinc amalgamated.			
	State of Metal.				State of Metal.			
	Pure.	Amalgamated.	Solid alloy with mercury.	Trace of metal in mercury.	Pure.	Amalgamated.	Solid alloy with mercury.	Trace of metal in mercury.
	Volts.	Volts.	Volts.	Volts	Volts.	Volts.	Volts.	Volts.
*Sodium ... ... a_1	...	...	...	...	1·56	...	1·10	...
" ... ... a_2	...	...	...	...	1·59	...	1·46	...
*Potassium ... ... b_1	...	...	...	...	1·68	...	1·06	...
" ... ... b_2	...	...	...	...	1·76	...	1·64	...
Zinc c	0·043	0·000	0·000	0·029 to 0·253	0·000	0·000	0·000	0·029 to 0·253
Cadmium... ... ... d	0·401	0·439	...	0·488	0·311	0·326	0·313	0·540
Tin ... ... e	0·571	0·599	0·596	0·626	0·509	0·515	0·510	0·531
Lead ... ... f	0·559	0·627	0·542	0·627	0·509	0·510	0·465	0·494
Iron ... ... g	0·487	...	...	...	0·497	0·486	0·407	1·258
Copper ... ... h	1·052	1·092	1·058	1·084	1·030	1·014	1·042	...
Silver ... ... j	1·326	1·335	0·971	1·363	1·169	1·275	1·275	...
Gold ... ... k	1·340	...	...	...	...	...	...	...
Platinum l	1·477	1·363	1·168	1·086	1·323	1·169	...	...
Brass ... ... m	...	...	...	...	0·888	0·592	...	...
Gun Metal ... ... n	...	...	...	...	0·597	0·637	...	...
Copper and Lead ... p	0·608	0·550	...	...	0·487	0·512	...	...
" " ... q	0·682 to 0·989	0·546	...	...	...	0·509	...	...
Silver and Zinc ... r	1·004	0·100	...	...	...	0·040	...	...
Gold and Silver ... s	1·133 to 1·204	1·172	...	...	1·030	1·084	...	...
" " ... t	1·260 to 1·210	1·088 to 1·220	...	...	1·208	1·087	...	...
" " ... u	0·882 to 1·067	1·061 to 1·170	...	...	1·107	1·085	...	...
Iron and Tin v	0·537	0·543	...	...	0·498	0·496	...	...
Platinum and Tin ... w	0·548	0·552	...	...	0·484	0·409	...	...
Lead and Zinc ... x	0·018	0·000	...	...	0·018	0·018	...	...
Silver and Copper ... y	1·070	1·047	...	...	0·898	0·996	...	...
Silver, Zinc, and Mercury ... z	...	...	...	...	0·004	...	...	...
Copper & Cadmium a^1	0·730	0·745	...	...	...	0·672	...	...
Brass by Electrolysis b^1	...	...	...	...	0·916	0·064	...	...
Mercury ... ... c^1	1·363	...	...	...	1·333	...	...	...

a_1 Potassium tested by melting potassium under paraffine wax, and drawing it up into a glass tube. After cooling, a wire was inserted into one end of the tube, and connected with one terminal of a condenser of small capacity (one third microfarad), and with one pair of quadrants of a Thomson's electrometer. The other poles of the condenser and electrometer were connected with a rod of zinc inserted in a beaker, with a solution of zinc sulphate. The end of the glass rod containing the potassium was then broken and dipped for an instant in the zinc sulphate, and the electro-motive force observed by noting the potential to which the condenser had been raised. The electro-motive force of potassium amalgum (solid) determined in the same way. The potassium amalgum (liquid) determined by introducing potassium amalgum, contained in a glass bulb, with a tube and tap blown on to the bottom, beneath the surface of mercury, the surface of the mercury being covered with zinc sulphate.

a_2 The electro-motive force given here is that determined in melted paraffine wax. The resistance of the circuit was very large, but a quite definite electro-motive force was noted—remaining fairly constant. The potassium was contained in a Clark's cell, and heated over iron gauze.

b_1 Sodium treated as above (a_1).

b_2 „ „ „ (a_2).

c, *d*, *e*, *f*. These metals amalgamated with diluted sulphuric acid, the solid alloys made by pouring mercury in a fine stream into the melted metals.

g, *h*. Metals amalgamated with sodium amalgum, the solid alloys with mercury made by pounding in a mortar, the pure metals precipitated by hydrogen with fragments of sodium, then adding mercury, and triturating the mass after it had fired. The sodium was dissolved out with water, and the excess of mercury pressed out in a vice, the alloy being wrapped in wash leather. Other alloys of these metals were made by electrolysis.

j. Silver amalgamated by dipping hot silver into mercury.

The solid alloy is "Arbor Dianæ," formed by precipitating silver by mercury from a solution of the protonitrate of silver.

l. Amalgamated by dipping red hot platinum foil into mercury, or boiling it in mercury. The solid alloys of platinum and mercury made by sodium amalgum from salts of platinum, or by boiling platinum black in a sealed tube with mercury, the tube being partly exhausted of air.

m to b_1 a^1 The alloys made by fusing the pure metals in a Fletcher's furnace under borax with charcoal at the top, amalgamated by sodium amalgum.

b_1 Brass precipitated from a solution, 1 gramme cupric sulphate, 8 grammes zinc sulphate, 18 grammes cyanide of potassium, and 250 grammes water, the solution boiling.* The anode a plate of brass made from pure metals, the cathode a piece of platinum foil or the surface of mercury.

p. Alloy mixed with large excess of lead.

q. Alloy mixed with large excess of copper.

r. Silver and zinc in equal proportions.

s. One part gold, three parts silver.

t. One part gold, four parts silver.

u. One part gold, five parts silver.

y. Silver and copper in equal proportions.

Where blanks occur in the table, they are commonly due to the fact that the electro-motive force was found to be too variable to allow of exact determination. *t*, *u*, *v*, of these alloys, the alloy one gold four silver appeared to have a definite electro-motive force, the others were very variable.

A few of the observations made only are recorded, and these were checked by several repetitions.

The figures given in the preceding table do not pretend to a greater degree of accuracy than about 3 per cent. With every care it was not found possible to rely on the figures in some cases

* See Text Books of Science, Electrolysis, by Gore.

to a greater degree of accuracy, owing to the very rapid polarization of the surfaces.

It will be seen by this table that the metals—potassium, sodium, cadmium, tin, and copper, are rendered more electro-negative by amalgamation. Lead and the more electro-negative metals are little altered. Iron and zinc are rendered distinctly more electropositive in dilute sulphuric acid. In zinc sulphate zinc is not altered by amalgamation.

Among the alloys, the most electro-positive metal present generally determines the position of the alloy.

The exceptions are, that zinc alloyed with silver, has an electro-motive force nearly the same as silver before amalgamation. After amalgamation, the zinc is dissolved out by the mercury, and the amalgamated alloy behaves as zinc.

Brass is curious in its behaviour, both before and after amalgamation the electro-motive force of the alloy is intermediate between that of copper and zinc. It was found, however, that after remaining amalgamated for some weeks, the alloy behaved nearly as zinc, showing that the zinc is ultimately but very slowly dissolved out by the mercury. These remarks apply to brass made by fusion. Brass made by electrolysis at once yields its zinc to mercury.

The third and fourth columns of the table show that (very nearly) mercury alloyed with any metal takes the position of the most electropositive metal that it may contain, independently of the amount of the metal dissolved in it, both in a solution of zinc sulphate, and in dilute sulphuric acid.

Many experiments were made on this point; for example, in dilute sulphuric acid, the electro-motive force

Between zinc and lead is 0·559 volts.

,, zinc and mercury containing $\frac{1}{104000}$th of lead 0·603 volts.

,, ,, ,, ,, $\frac{1}{35000}$th ,, 0·582 ,,

,, ,, ,, ,, $\frac{1}{700}$th ,, 0·569 ,,

Again, zinc compared in the same acid with mercury, containing $\frac{1}{35000}$th part of lead, and $\frac{1}{190000}$th part of zinc, gave an

electro-motive force of 0·193 volts a few seconds after the zinc was added, afterwards the electro-motive force steadily increased.

In 5 minutes	it	was	0·415	volts.
,, 10	,,	,,	·449	,,
,, 15	,,	,,	·486	,,
,, 20	,,	,,	·509	,,
,, 21	,,	,,	·508	,,
,, 22	,,	,,	·610	,,

Showing that the trace of zinc added was gradually dissolved out, the lead existing in larger quantity remained longer, but this, too, is ultimately eliminated. Mercury, containing $\frac{1}{100000}$th part of lead, becoming nearly pure in 24 hours. Mercury, containing $\frac{1}{35000}$th part lead, and $\frac{1}{5440}$th part zinc, gave an electro-motive force 0·048.

With three or more metals present in the alloy, the most electro-positive determines the electro-motive force, For instance, mercury, containing $\frac{1}{5440}$th of zinc, gave an electro-motive force 0·031, the addition of an equal quantity of lead, and of $\frac{1}{770}$th part of tin did not alter its value, nor did the addition of cadmium or any more electro-negative metal.

The mercury was allowed to stand in the solution. After three months the electro-motive force had risen to 0·328, showing that cadmium was still present.

In a solution of chloride of ammonium, 1 part of the crystallized chloride to 50 of water, mercury containing $\frac{1}{790000}$th part of zinc gave an electro-motive force 0·227 a short time after the addition of the zinc.

							Volts.
The electro-motive	force	rose	in	1 minute		to	0·285
,,	,,	,,	,,	5 minntes		,,	0·374
,,	,,	,,	,,	10	,,	,,	0·725
,,	,,	,,	,,	50	,,	,,	0·898
,,	,,	,,	,,	56	,,	,,	0.982
,,	,,	,,	,,	77	,,	,,	0·984
,,	,,	,,	,,	24 hours		,,	1·187

shewing that the zinc was gradually but slowly eliminated, the last value given corresponding to pure mercury.

In a solution of zinc sulphate, mercury alloyed with zinc in very small quantities becomes nearly zinc for instance. In a certain solution of zinc sulphate zinc compared with mercury pure electro-motive force = 1·186. Zinc compared with mercury with zinc in proportions given below gave a lower electro-motive force, as follows:—

Zinc 1 part,	mercury	23,600,000	parts	=	1·179
„	„	11,800,000	„	=	1·080
„	„	7,530,000	„	=	0·655
„	„	5,900,000	„	=	0·513
„	„	4,720,000	„	=	0·399
„	„	3,930,000	„	=	0·257
„	„	3,370,000	„	=	0·228
„	„	2,700,000	„	=	0·214
„	„	2,620,000	„	=	0·214
„	„	1,800,000	„	=	0·214
„	„	400,000	„	=	0·134
„	„	200,000	„	=	0·124

With cadmium similar results were obtained, for example:—In a particular solution of zinc sulphate to which some hydrated oxide of zinc had been added to neutralise any acid which might exist,

Pure mercury against zinc gave electro-motive force = 1·232.

Cadmium 1 part,	mercury	5,700,000	parts	=	1·146	volts.
„	„	3,160,000	„	=	1·090	„
„	„	1,912,000	„	=	0·626	„
„	„	1,450,000	„	=	0·572	„
„	„	1,167,000	„	=	0·512	„
„	„	977,000	„	=	0·475	„
„	„	842,000	„	=	0·464 ?	„
„	„	742,000	„	=	0·475	„
„	„	662,000	„	=	0·472	„
„	„	598,000	„	=	0·462	„

Cadmium 1 part, mercury		407,000	parts	= 0·456	volts.
,,	,,	314,000	,,	= 0·451	,,
,,	,,	153,000	,,	= 0·438	,,
,,	,,	101,400	,,	= 0·428	,,
,,	,,	76,200	,,	= 0·421	,,
,,	,,	61,100	,,	= 0·420	,,
,,	,,	51,100	,,	= 0·420	,,
,,	,,	24,240	,,	= 0·410	,,
,,	,,	6,280	,,	= 0·396	,,
,,	,,	3,580	,,	= 0·389	,,
,,	,,	2,515	,,	= 0·386	,,
,,	,,	1,942	,,	= 0·387	,,
,,	,,	1,538	,,	= 0·378	,,
,,	,,	1,339	,,	= 0·376	,,
,,	,,	1,158	,,	= 0·380	,,
,,	,,	963	,,	= 0·378	,,
,,	,,	873	,,	= 0·374	,,
,,	,,	836	,,	= 0·374	,,
,,	,,	332	,,	= 0·366	,,
,,	,,	148·7	,,	= 0·334	,,
Amalgamated cadmium				= 0·326	,,
Unamalgamated	,,			= 0·311	,,

The electro-motive force given is that observed as quickly as possible after the addition of the zinc, the reading altered rapidly until the quantity of zinc amounted to about one part in two millions, when they remained fairly steady.

To determine the point whether hardening a zinc plate altered its position in the scale of electro-motive force to any marked extent, six pieces of very pure zinc, rolled to a thickness of about 0·05 m. m., were compared together, and no sensible electro-motive power was found to exist between them.

Three of the plates were then sealed in a thin glass tube, and annealed by boiling the tube in mercury for two hours. No electro-motive force was observed between the plates that had been annealed and those that had not, either before or after amalgamation.

As might be expected, the results obtained in solutions containing salts of mercury, or other more electro-positive metal, were very different.

Mercury containing $\frac{1}{200,000}$th part of zinc compared with zinc gave electro-motive force 0·124 ; the addition of sulphate of mercury on the surface of the mercury raised the electro-motive force at once to 1·498 ; the further addition of zinc to one part in 40,000 lowered the electro-motive force to 1·485 volts only.

An alloy of mercury and zinc, giving with zinc an electro-motive force 0·0455 volts, was made, and various salts added on the surface of the mercury, the electro-motive force observed was—

Mercury and zinc	0·0455
Mercury with protoxide of mercury on the surface of the mercury ... ... =	0·0444
Mercury with binoxide of manganese ... =	0·0473
,, ,, sulphate of lead ... ... =	0·0473
,, ,, chloride of lead ... ... =	0·0473
,, ,, chloride of silver ... ... =	1·260

The chloride of silver disguising the effect of the zinc. That the zinc was not wholly eliminated, was proved by removing the chloride from the surface of the mercury, when the electro-motive force again fell.

The same experiment was then repeated with other salts and acids.

Mercury containing $\frac{1}{813}$th part of zinc with chloride of silver resting on the surface gave at first an electro-motive force of 0·822 volts. In 24 hours this had risen to 1·047 volts.

The addition of		sulphate of mercury	altered E.M.F. to	1·067
,,	,,	strong sulphuric acid ,,	,,	0·992
,,	,,	crystal of chromate of potash	,,	1·006
,,	,,	solution ,, ,,	,,	1·083
,,	,,	crystal of chromic acid	,,	1·452

A crystal of chromic acid dropped through the solution on the surface of mercury, containing even a considerable proportion of zinc, immediately covers the surface with chromate of mercury,

and an electro-motive force exceeding that of Clark's cell is produced. On stirring the mercury, the electro-motive force at once again falls. A solution of chromic acid has little effect. A crystal of sulphate of copper raises the electro-motive force of impure mercury to that of copper if the impurities are not in too large amounts.

That the effect of zinc in lowering the position of mercury in the scale of electro-motive force is not due to the zinc floating on the surface of the mercury, was proved by placing the impure mercury in a glass tube, the bottom closed with a linen cloth, the under surface of the mercury alone being wetted by the solution. The electro-motive force was the same as when the upper surface of the mercury was exposed to the liquid.

In further illustration of this point, it was found that the surface of mercury containing a small quantity of zinc exposed to the liquid could be greatly increased without altering the electro-motive force.

A volume of 50 c.c.m. of mercury containing $\frac{1}{2,000,000}$th part of zinc, was poured into a flat plate glass cell with a partition across the middle, and covered with a solution of zinc sulphate. The electro-motive force between the mercury and zinc was 0·216 volts, on removing the partition and allowing the mercury to flow over twice the surface, no change in the electro-motive force was apparent. All portions of the surface of the mercury were at the same potential. This was proved by separating several portions by a glass tube ground plane at the end, and with a small hole blown in it above the surface of the mercury to admit the zinc sulphate. The tube was placed over various points of the surface of the mercury, and pressed down until the flat end rested on the bottom of the cell, and the electro-motive force between zinc and the mercury enclosed in the tube observed. It was found to be the same at all points of the surface of mercury.

Contact with the mercury enclosed in the tube was made by means of a platinum wire sealed in a thin glass tube, with the end projecting 1 m.m. from the lower end.

In order that a salt should disguise the effect of the zinc on the mercury, it was necessary that it should cover its surface completely.

The rapidity with which the surface of some of the metals

recovered their normal state after polarization was measured in the following manner :—A plate of the metal and one of zinc were placed in a rectangular cell of plate glass, together with a second plate of amalgamated zinc, one of the zincs and the electro-negative metal were connected for a given time, and the connections were arranged so that immediately on breaking contact the second plate of zinc could be compared with the polarized electro-negative plate.

The most striking results were obtained with mercury in zinc sulphate.

Two troughs exposing a surface of about 9 square c.m. each were filled, one with pure mercury, the other with mercury containing $\frac{1}{115,000}$th part of zinc. The electro-motive force between the pure mercury and zinc was 1·2288 v., that between the impure mercury and zinc was 0·086. The troughs of pure and impure mercury were then connected for 5 minutes by a piece of platinum wire sealed in glass, except at the end where it was under the surface of the mercury. On breaking contact the pure mercury was compared with zinc with these results :—

Time after breaking contact	Electro-Motive Force.
½ min.	0.102
1 „	0·106
2 „	0·108
3 „	0·110
4 „	0·111
5 „	0·113
6 „	0·114
7 „	0·116
8 „	0·118
9 „	0·120
10 „	0·122
11 „	0·124
12 „	0·126

The surface of the pure mercury after 12 minutes still remaining nearly as electro-positive as zinc.

Again, a short length of platinum wire d0·5 m.m. was taken, and one end amalgamated with pure mercury for a length of

2 m.m. only, and the amalgamated end dipped into mercury containing $\frac{1}{115,000}$th part of zinc.

The clean end of the platinum wire was then dipped into a trough of mercury 9 square c.m. in surface, containing 12 c.c.m. of pure mercury, the amalgamated end being under the surface of the zinc sulphate with which the mercury was covered. On removing the platinum wire, the surface of the pure mercury was found to be completely polarized, results similar to those already given being obtained.

A surface of mercury polarized by short circuiting it with a zinc rod for a few minutes, is capable of polarizing another equal surface of mercury. For example, two small troughs of 9 square c.m. surface were placed in a flat dish filled with a solution of zinc sulphate, together with two rods of amalgamated zinc and terminal wires attached. Call the mercury troughs T_1 and T_2, and the zincs Z_1 and Z_2, the electro-motive force between T_1 and Z_1 or Z_2 was 1·263, between T_2 and Z_1 or Z_2 1·212, a little lower from momentary contact having been inadvertently made between the terminals of Z_1 and T_2.

Z_2 and T_2 were connected for about 5 minutes; on breaking contact, the electro-motive force between them was 0·050. T_1 and T_2 were then joined for 12 minutes; after breaking contact, T_1, compared with Z_1, gave an electro-motive force of 0·178, rising rapidly.

That the surface of T_2 retained a considerable quantity of hydrogen was shown by dropping chloride of silver on the surface; ·088 grammes of silver chloride were added, and still the surface was not completely depolarized.

That the effect was due to the condition of the surface of the polarized mercury was proved by adding some of the polarized mercury taken up in a pipette to pure mercury; the electro-negative condition of the pure mercury was not at all affected.

A plate of copper, cleaned by dipping in strong nitric acid and then immediately plunging in water, was polarized in a similar manner. The plate before polarization gave, with zinc, an electro-motive force = 0·945. After polarization, the first value obtained on breaking the circuit was 0·032; after 1 minute, it was 0·188;

after 2 minutes, it was 0·230. A small crystal of sulphate of copper was then dropped on to the plate of copper. In 15 seconds, the electro-motive force rose to 0·305; in 5 minutes, to 0·967. A bright spot on the copper showed that a sensible quantity of copper had been precipitated from the sulphate by the action of the hydrogen. A plate of iron recovered its normal state much more quickly than one of copper. A certain plate of iron gave with zinc, an electro-motive force 0·497 in zinc sulphate. Immediately on breaking contact after the iron had been polarized by contact with a zinc plate, the electro-motive force was found to be 0·310. After amalgamation, the surface of the iron retained the hydrogen longer, but not so long as a surface of pure mercury. Amalgamated iron, after polarization as before, gave an electro-motive force immediately after breaking contact of 0·2020; in 10 seconds this rose to 0·270; in 30 seconds to 0·367.

Two kinds of iron were tried, one an ordinary screw, the other a plate made by placing commercially pure iron precipitated by hydrogen in a crucible with a flux; raising it in a Fletcher's furnace to nearly welding heat, and then hammering the hot iron into a plate. Similar results were obtained with both specimens.

Other metals were tried, but none retained the hydrogen with the tenacity of mercury. In dilute sulphuric acid the hydrogen was not retained with the same ease by any metal tried. Pure mercury gave in a certain mixture of sulphuric acid and water, an electro-motive force of 1·270 volts. After polarization by contact with a zinc plate the electro-motive force observed within a few seconds of breaking contact was 0·648 volts, in 15 seconds the electro-motive force had risen to 1·134, and within the minute to 1·216.

A copper plate was nearly depolarized in dilute sulphuric acid within 15 seconds. Platinum also gave up its hydrogen with much greater facility than mercury. For instance, in dilute sulphuric acid (1-20) the electro-motive force between zinc and platinum was 1·478, after short circuiting the platinum with the zinc for a certain time contact was broken, and the electro-motive force first observed between the platinum and zinc was 0·783. This rose in

1st	minute	to	0·832	
2nd	,,	,,	0·858	
3rd	,,	,,	0·858	
4th	,,	,,	0·937	solution stirred.
7th	,,	,,	1·026	and ultimately to 1·134

The electro-motive force did not increase beyond the last value. Still the surface of the platinum had become depolarized by this time; the low electro-motive force observed was due to the sulphate of zinc formed by the action of the acid on the zinc.

The surface of the platinum was cleaned by boiling in nitric acid, heating to a red heat, &c., but the full electro-motive force was not restored until the solution had been changed. Still the figures given show that platinum becomes depolarized in dilute acid quickly.

In a saturated solution of zinc sulphate analogous results were obtained.

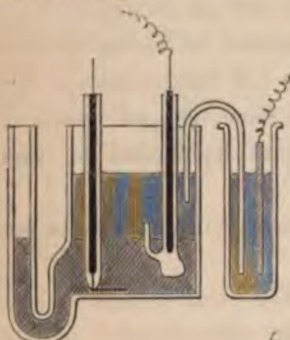
Platinum compared with zinc the electro-motive force = 1·315 volts; after short circuiting, the first value obtained after breaking contact was 0·269 volts.

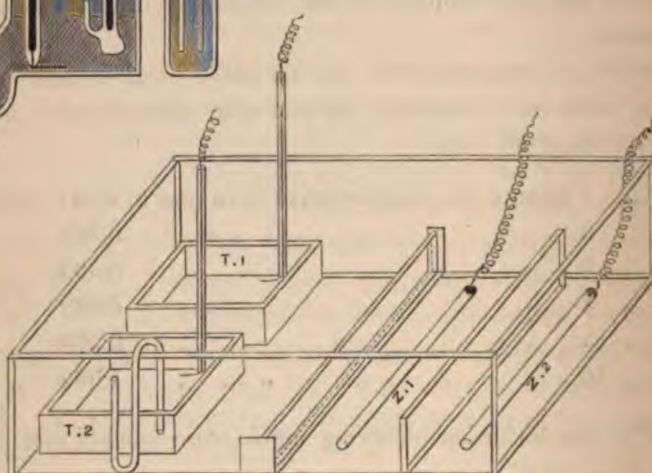
In 1	minute	the electro-motive force	rose	to	0·341	volts.
,, 2	,,	,,	,,	,,	0·564	,,
,, 3	,,	,,	,,	,,	0·809	,,
,, 4	,,	,,	,,	,,	0·912	,,
,, 5	,,	,,	,,	,,	0·970	,,
,, 10	,,	,,	,,	,,	1·062	,,

The depolarization occurring much more quickly than in the case of mercury.

It will be seen by some of the values given above that it was quite essential to prevent any mercury that had been in contact with any of the more electro-positive metals being used to amalgamate one of the more electro-negative metals, or employed for comparison as a standard. Commonly to prevent this occurring during an observation, the amalgamated plates, or one of them, was enclosed in a glass tube, with a small bulb blown at the bottom, and an S shaped tube of smaller section blown on to the larger

tube a little above the bulb. In this way free access is given for the solution to reach the amalgamated plate, and at the same time the solution can be stirred, and the plates to be compared removed without danger of dropping the mercury from the amalgamated plate on to the second plate, or into the glass vessel containing the solution in which the metals compared were placed. A Clark's cell, v. Ph. Trans., 1874, was found a convenient vessel for comparing mercury with other metals, or a glass funnel with the tube bent up.





When two solutions were employed they were placed in different vessels, and connection made by a glass U shaped syphon of small bore (2 m.m.s.). When large surfaces of mercury were exposed to the action of the electrolyte, rectangular cells of plate glass were used, contact with the mercury was made either by a platinum wire, of which the length that was not in the mercury but passed through the electrolyte was sealed in a glass tube, or by a syphon *of small bore*, one end dipping under the mercury.

From these experiments, we conclude that within narrow limits mercury alloyed with any number of metals takes the place in the scale of electro-motive force of the most electro-positive metal that it may contain if the amount of the electro-positive metal present is not less than about $\frac{1}{1,000,000}$th in weight of the mercury. This seems explained by the facility with which a mercury surface becomes polarized, and the tenacity with which it retains the hydrogen.

It has been shown that nearly the same result occurs whether one millionth part of zinc is added to a mass of mercury, or if the trace of zinc is placed on one end of platinum wire and the other end dipped into the mercury. The electro-motive force of the mercury, platinum, and zinc treated as one plate of a cell, is nearly the same as that of the impure mercury so treated. Thus, also, if any zinc or other impurity exists on a portion of the surface of a mass of mercury exposed to an electrolyte, the whole surface will fall nearly to the potential of the zinc or other the most electro-positive impurity present.

Further confirmation of this seems to be given by the fact that in a neutral solution of zinc sulphate slight traces of zinc and lead are eliminated from mercury after a time, presumably by the electrolytic action described. Again, when the surface of mercury containing traces only of zinc, say, is covered with a salt such as proto-sulphate of mercury, the effect of the zinc is lost. If polarization is the important element that we suppose it to be, it would appear that in the present case an electro-motive force exists between any portion of the surface not contaminated with zinc and any portion containing zinc. Pure mercury will be reduced from the salt on the portions of the surface free from zinc, and covered only with nascent hydrogen, and the action will extend until the whole surface may be covered with pure mercury, if the quantity of zinc present is not sufficient for the surface of the mercury to be supplied by diffusion with the zinc from the mercury below the surface more rapidly than it is dissolvod by electrolytic action.

The nascent hydrogen may commence the action, for we have found that a crystal of cupric sulphate dropped on a polarized plate of copper deposits pure copper on the portion of the plate it touches,

and the whole surface of the plate is depolarized, and a trace of sulphate of mercury dropped on the polarized surface of mercury depolarizes it.

In the cases of lead, zinc, and cadmium, it appears that the presence of $\frac{1}{400,000}$th part of impurity lowers the electro-motive force between the impure mercury and the metal present to about 0·117 volts.

Again, if mercury containing zinc in very small quantities is agitated in water, a black powder separates out. May this be due to the fact that fresh surfaces of mercury being continually presented to the liquid, the effect of polarization is diminished, and electrolytic action between surfaces containing more zinc and surfaces containing less being facilitated, the oxidation of the zinc is hastened. M. Regnault's explanation of the change in electro-motive force when a metal is amalgamated seems quite satisfactory. In this case there is a saturated solution of the metal in mercury. If in forming the solution sensible heat is lost, the combination formed has gained energy, and more energy may be developed by oxidization of the zinc, which action supplies the energy maintaining the current. In other words, the metal becomes more electro-positive.

The reverse action appears explained in the same way. When the combination of the metal with mercury developes sensible heat, the combination has become more inert, and may develop less energy on the separating out and oxidization of the electro-positive metal.

With these exceptions, mercury appears nearly inert. Any metal may be alloyed with mercury to any extent; the mercury may be in enormous excess, or a saturated solution of the metal in mercury may be made, or the mercury may form with it a solid alloy, the result is, within narrow limits, the same as though the mercury were not present.*

We have already drawn attention to the result of amalgamating alloys. Among those we tried, the mercury at once dissolved out the most electro-positive metal present, except in the case of

* Potassium and sodium are exceptions.

brass, the zinc of which was not separated from the copper by the mercury for some weeks, except in such small quantities that the surface of the mercury was not supplied with zinc with sufficient rapidity to maintain it at the potential of zinc. We may mention that the most electro-negative electrode tried was platinum in strong sulphuric acid with pure peroxide of manganese. The most electro-positive potassium in potassium sulphate. The first was 2·5 volts negative to zinc, the latter 1·694 positive to zinc in zinc sulphate. It appears also that in practice the variations of electromotive force in a battery are more likely to arise from the electro-negative than from the electro-positive plate, for the effect of impurities in the electro-positive plate are greatly masked by the rapid polarization of surfaces of metals negative to the plate and in contact with it. This is not the case to the same extent with the electro-negative plate, impurities positive to the plate tend to reduce it to the potential of such impurities.

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OF THE

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The Seventy-seventh Ordinary General Meeting of the Society was held on Wednesday Evening, April 9th, 1879—the President in the Chair.

The President: Before proceeding to the adjourned discussion on Mr. Sivewright's paper I have to ask you to give a few moments to a few remarks from Dr. Siemens.

Dr. C. W. Siemens: We have here this evening a visitor from France —M. Hospitalier—who has introduced to my notice, and, I believe, to several other Members of the Society, a very interesting modification of the phonograph. It is a very simple apparatus, and I have requested M. Hospitalier to be good enough to bring the instrument here this evening, in order to give the Members present an opportunity of seeing it. I may describe it in a few words. It consists in the first place of a lead wire upon which certain impressions are engraved. Those engravings are produced in the following manner:—The phonograph, armed with a knife edge instead of a point passing over a sheet of tinfoil, passes over a bar of stearine. This bar of stearine receives the phonographic impression. Upon this bar being covered afterwards with a conducting substance, such as plumbago, a deposit of copper is made by an electrotyping process, and into this mould lead wire is pressed either by being beaten down upon it, or being pressed in any convenient manner upon it, and thus stereotype impressions are produced for the phonographic moulding. On passing this little disc of paper over these indentations the word to be repeated is distinctly heard

on placing to the ear this little funnel of cardboard, which is connected to the paper disc by a leaden wire.

[The effects having been experimented upon by the President.]

The PRESIDENT: I think from what I have heard it would be a little difficult to make out the words unless one knew what was coming. When you are told the sound represents two or three words then you may make them out. I do not think I should be able to distinguish the words myself otherwise.*

Dr. SIEMENS: I may add that the apparatus is the invention of Mons. Lambrigot, in the telegraph service of France, and with your permission I will propose a vote of thanks to M. Hospitalier for having brought it before us.

This was unanimously agreed to.

The PRESIDENT: The next business is the adjourned discussion on Mr. Sivewright's Paper "On South African Telegraphs," I believe Mr. Bell has some remarks to make.

Mr. BELL desired to bring to the notice of Members present an insulator designed by Dr. Campanema, which he had placed on the table, as it appeared to possess some excellent features. On the line wire there was cast a spherical ingot of some metallic alloy, which fitted into a socket in the top of the insulator, and formed a secure fixing for the wire. The metallic or wire surface in contact with the insulator being very small diminished leakage; and another good point was that it would be free from a serious defect that attends the use of the common wire binding. When the galvanizing wears off the line and binding wire, an oxide of iron soon covers the surface of the insulator and causes great leakage. With regard to the breakage of insulators by lightning discharge on the South African lines, as described by Mr. Sivewright, he had known similar cases resulting from severe thunder storms. An earth-wire attached to a pole in the usual way does not protect the insulator. It presents but a small surface to a charged cloud compared with the line-wire; hence the latter takes the discharge

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* M. Hospitalier discovered afterwards that the reason why the action of the instrument was not so perfect as usual was on account of a partial disconnection in the leaden wire.

and breaks the insulators. To save the insulators, the earth-wire should be brought near the line-wire.

Mr. SPAGNOLETTI: From what has fallen from Mr. Bell, I would say, with regard to the binding in of line-wires to insulators, that bad binding is a serious matter; if it is not well and tightly done, so as to keep the line-wires well up to the insulators, the wire, from the hammering and chafing it becomes subject to from vibrations set up by the wind and other causes will in time become quite flattened, and I have known several cases where the wire has been quite indented and weakened to half its strength by this process. The result is that on the first frost, this beaten and weakened portion cannot stand the contraction, and a breakage follows. After having given but an indifferent description of Mr. Oppenheimer's insulator to the last meeting, I wrote to that gentleman to send me one of his insulators that I might be the better able to describe and show it to this meeting, but I received a letter from him stating that Mr. Preece had been in communication with him on the same subject, and that he had sent some of his insulators to him. We shall, therefore, no doubt have the benefit of seeing them to-night. I mentioned at the last meeting the preservation of the timbers which Mr. Sivewright refers to as growing in abundance in the Northern forests. The class best calculated for telegraph poles, he states, contains a very great quantity of sap, and that sometimes the sap does not go down. I still maintain my opinion that the process of kyanizing is the best calculated for the preservation of such descriptions of timber. Faraday tried many experiments both on timber and fabrics, and with the preservative powers of kyanizing he was quite satisfied. This system was adopted on the Great Western Railway, and the preparation of the timber used for viaducts and bridges—and there were and are many such structures on this railway—proved very successful, some of the timber lasting quite sound for 20 to 30 years, although exposed, and in some cases exposed to the action of salt water. The difficulty of transit of stores is a question on which much stress has been laid, both as to inconvenience and cost, and also the deleterious effects of the climate upon bullocks and horses. I would suggest the utilizing of elephants for this work, if it is possible. I do not

know whether they would be subject to the same fatality from the tsetse flies, certainly not from climate; and as in India and other countries, elephants are found to be most sagacious and very useful, I do not see why they should not be trained and employed for these and other purposes in Africa, where numbers of them exist.

Mr. LATIMER CLARK: With reference to this lightning spike I may mention that I sent to Colonel Robinson, in India, a number of insulators with the lightning point Mr. Bell has referred to which goes nearly to the edge of the insulator, but I have never heard what the result of the trial of them was.

Mr. W. H. PREECE: It is well known that for some time past I have taken considerable interest in the progress of telegraphy in South Africa, and it was through my instrumentality that Mr. Sivewright was sent out there. We are all glad that he has been in a position to favour the Society with the excellent and admirable paper which was read at our last meeting. The chief interest of that paper really, like that of a lady's letter, was in what one may call the postscript, and that is the question raised as to the relative merits of a land line and a cable line. Now that imperial necessity has solved this question, and now that only yesterday the *Kangaroo* left these shores with a cable to be laid between Natal and Mozambique, it would seem almost unnecessary to discuss the question as to the relative superiority, or necessity, of a land line or a submarine cable. But all telegraphists know that it does not do to depend upon one string to one's bow. The whole system of the Postal Telegraphs of this country has been laid out with the idea of giving to every station an alternative route, and this idea has been carried out at the Cape, as pointed out in the paper, and as an alternative route the question of land lines sooner or later must again crop up.

Now we have heard in the course of the discussion on this paper rather strong opinions expressed against the practicability and feasibility of land lines. One gentleman of experience in India and the Cape has said it would be madness to attempt to carry a land-line through Central Africa. On the other hand, we had the opinion of the Crown Agent for the Colonies, *Mr. Sargeaunt*, only recently returned from South Africa, who said

his opinion against land-lines was changed by being brought into contact with those in the Cape who knew the country and had had experience there.

Now, in solving a question of this kind, experience on the spot is better than fireside philosophy. Fireside philosophy means the philosophy of those who "sit at home at ease," and evolve from their inner consciousness difficulties and dangers which may never exist. The difficulties, dangers, and obstacles raised against land-lines are numerous. In the first place, the folly of carrying a line through an unexplored country has been mentioned; but what is the folly of carrying a line through an unexplored country compared with the folly of laying a cable through an unfathomable deep? We know more about the surface of the land than we do about the bottom of the sea. Again, it has been pointed out that it is a serious objection to a land-line to carry it through a country inhabited by natives and tribes of lawless character—persons who look upon a line of iron through their country in somewhat the same light as the roughs of London or the boors of Yorkshire would look upon a line of gold between London and Newcastle—but no less an authority than Colonel Grant has said the only object of savages in stealing wire is to make trinkets; and if you supply them with those trinkets that are articles of vertu in their eyes, you stop the tendency they have to steal the wire.

Again much has been said about the unhealthiness of the climate. No doubt the climate of Africa in some parts is extremely unhealthy, but to judge from the physical appearance of gentlemen recently returned from that country, though the effect of the climate may be temporarily bad, the permanent result does not seem to be serious; and we know that Englishmen of all others can stand climate everywhere. The most serious difficulty against land lines is transport, and the great cost of it, but Mr. Sivewright points out that the proposed 2,400 miles of line is so broken up into sections of about 800 miles each that in no place is it necessary to carry stores to a greater distance than 400 miles. Though Mr. Mayes says it would require 124,000 coolies to transport a line of 2,400 miles, he quite forgets that it might be broken *up into these sections,* when the stores could be carried by

a comparatively small number of men. Again, we are told of the difficulties of land lines with regard to elephants, buffaloes, and other animals that rush up against the poles upon the principle on which the Scotch are said to "bless the Duke of Argyll." We have heard also of the tormenting effects of flies upon horses, and mules, and oxen employed; but there are parts of the country where these difficulties are not met with, and where they are by no means insurmountable. Again, we have heard of the difficulties of swamps and jungles; we have heard from Mr. Mayes of rank vegetation which in a few weeks obliterates all traces of roads; but vegetation is met with in countries where telegraphs exist, and practical telegraphists have means by which this can be done away with. Lastly, we have heard of the effects of forest fires, but we are at the same time told that the action of the fire is so rapid that it only chars the outside of the pole and does not burn it down. These are the difficulties brought forward against land lines.

On the other hand how stands it with regard to a cable? Experience has taught us that the life of a submarine cable is limited: the life of a cable is not everlasting. We know when the Atlantic cable was first projected it was thought when once it was laid it would last for ever. In fact well known Members of the Society have said the life of the Atlantic cable could not be estimated at less than 100 years. We know that the cables of 1865-66 have come to grief: in fact they never will be seen again. It has been said by so eminent an authority as Professor Huxley that the bottom of the Atlantic is so smooth that a waggon might be driven over it from Ireland to Newfoundland: but the experience of last year has been to show that the bottom of the Atlantic is in places as rugged as that of the Channel: that the cable corrodes and grows weaker, and at points where it is suspended it is liable to be broken. Again, the life of a cable is dependent upon the perfection with which it is manufactured. Though every inch of it may be watched with anxious eye in the process of manufacture, yet little faults may pass unnoticed, and these may lead to faults which may ultimately cause a breakdown. Bad joints may escape the most careful jointer: a bilious attack or a little drink may so affect the *jointer's skill* that he may put in bad joints which lead to faults.

Again, there are faults which are put into a cable in the act of laying; and we know that cables laid across the Atlantic have met with accidents, through pieces of wire accidentally left on the coil being driven into the cable as it passed over the sheaves in paying out. There may be evils which beset a line of telegraph on shore, but we know at the bottom of the sea we have the teredo and other little creatures whom nature has provided with powerful organs of penetration, and has imparted to them a taste for gutta percha, and these unprincipled brutes, with no anxiety to tap the wire and learn the latest news, still get very near the wire, but do not always get near enough to know what is passing. There are liabilities to a cable of other kinds of accidents, such as sunken rocks falling across it. While there are difficulties in laying wires along an unexplored country there are also difficulties in laying a cable along the unexplored bottom of the sea: so that with all the difficulties surrounding a land line, compared with the difficulties surrounding a cable, there is not so much to be said on the side of one as against the other. In truth, perhaps, the cable involves the greater difficulties of the two passing these shores, which of all others have been surveyed the least, abounding with coral reefs and coral life—a shore swept by tremendous monsoons and by fierce currents, and where the cost of maintenance must necessarily be very great.

Nevertheless, notwithstanding all these difficulties, the question has been solved. If it had been a question of money, in construction and maintenance, I think it would have been a toss up between the two. Mr. Sivewright, in his natural anxiety to obtain a land-line, has perhaps under-estimated the cost, while the cable companies may have over-estimated it—the general result being that there is not much difference in cost, probably, between the two. However, as I have said, the question has been solved; but, sooner or later, the question of alternative routes must come up again, and I do not see why, sooner or later, a land-line should not be constructed across the Continent of Africa.

With regard to the paper of Mr. Sivewright, some of the points of detail are interesting to us. One question, at least, Mr. Sivewright has *solved with regard to* South Africa—that is, the question

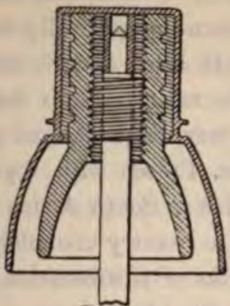
of the relative value of wood and iron for the purposes of posts. He has shewn that wood abounds in that country. As I gather from the sketches he has sent and from the description he has given, the sneezewood poles must present a hideous appearance. We all know that iron poles are in themselves very pretty things, and those which have been sent out are of very perfect design, the earlier portion being manufactured by Messrs. Siemens, and the later ones by Muirhead & Co., and they are as perfect as iron poles can be; but it is pointed out that buffaloes and oxen rub against them and knock them over, whilst they are also damaged by waggons coming in contact with them.

Reference has been made by Mr. Spagnoletti to the kyanizing of timber for telegraph poles. We have tried that. We know it is good for sleepers on railways and for bridges, but telegraph poles are subject to a species of decay which other timber is not subject to. The great point of decay is at the line between wind and water, just where the pole touches the ground, and this goes on generally in proportion to the variations of temperature. Whether it arises from the germs of vegetation or any other cause, it is there that telegraph poles fail, and kyanizing has been of no use in stopping the decay. There was a line erected on the South Western Railway, formed of kyanized poles, while another was formed of poles treated by the process called Burnettizing. In fact, every known process has been tried, and the only one that has come to the front is creosoting.

Mr. Sivewright has alluded to the question of insulators, and pointed out that the insulators had suffered very much from the effects of lightning, the four that suffered most being that of the brown earthenware, which smashed up a good deal; and allusion has been made to Mr. Oppenheimer's insulator, which has been designed practically as a lightning protector. This [exhibiting] is an Oppenheimer's insulator, and the peculiarity of it is the bolt screws into the insulator, and it is constructed in a way I can show you on the board.

[Illustrating by drawing on the board] That is the earthenware cup, and into that is fixed an iron pin. On the top of the iron pin *is an ebonite* washer, and the pin itself is finished off to a point

there. Over all is an iron hood. You see there is an air space, and the air space must be kept dry by the iron cap. The wire being bound to this iron cap the lightning instead of smashing



through the earthenware will flow across this air space and escape to earth. Of course this is mere theory; whether it answers practically or not I do not know, but it has been sent to me by Mr. Oppenheimer for the inspection of the Society, and for that reason I have brought it before you.

Another point alluded to by Mr. Sivewright was the question of the use of compound wire; and he mentioned that the great desideratum for Colonial telegraphs was a wire which should possess the conductivity of No. 4 gauge and the strength of No. 8, and be as light as possible. Our own experience of compound wire has been a disastrous one. It was composed of two materials of unequal expansion. The result has been that the whole wire became corroded, and it completely came to grief, so that at the present moment we are anxious to see some kind of compound wire that will meet our requirements, but that has not yet been produced. We have now under trial a form of wire by Wallace, of Ansoma, Conn., an American maker, and here are some specimens of it for inspection. It is steel wire, coated with a thick layer of copper deposited by electro-chemical process; but as far as we can judge at present it does not come up to our requirements, inasmuch as its resistance is as great as No. 8 gauge, while its breaking strain is very much less. I will only further express my great pleasure to find that my friend and right hand, who was with me so long, has made such *progress with* regard to telegraphs in South Africa, as

not only to prove a credit to telegraphs generally, but an ornament to the Society of Telegraph Engineers (Applause).

Sir CHARLES BRIGHT: Mr. Preece has given his views upon the comparative value of a submarine cable and a land-line for Central Africa, and he sums up generally that the advantages seem to be about equal on both sides, and he estimates the cost in each case would be about the same. As to the first cost it might be about the same, or not very different, but as to the relative advantages I differ from Mr. Preece with regard to the comparative merits of a submarine line to South Africa at the present time, and of a land-line through a country unexplored as it is and without roads. Mr. Preece speaks of a submarine cable laid in unfathomable seas, and the likelihood of damage from rocks in shallow water. I do not know of any unfathomable seas, and there are no special conditions of difficulty or liability to injury in the route referred to. I had not the opportunity of hearing the paper read, therefore I shall not go into the details of it. My own opinion is whatever the physical difficulties may be in the way of a land-line at the present moment, the moral difficulties of the people, through whom the line is to be taken, are much greater. Mr. Preece apprehends danger at first from the wire being cut away by the natives to make trinkets with, but that any Birmingham jeweller could supply a sufficient quantity of those articles to induce the people to discontinue the cutting of the wires. My own experience in foreign countries has been that the wires have been generally cut by the natives to make slugs for their guns, and various small iron articles of utility. I do not think civilization would follow the track of the telegraph as rapidly as it is suggested, or that the interior of the part of Africa spoken of is ready for a land-line, though perhaps half a century later it may be.

I remember when I was engaged upon the Persian Gulf line in 1863-4, the line having been finished to Mesopotamia it could not be continued to Bagdad because the Arabs, who were in revolt against the Turks, cut the wires as fast as they were put up, so that it was not till after a period of seven months that the communication could be completed: this was through a country which might be *called civilized* in comparison, with regular steamers passing up

and down the river, and with ample means of transport at our command.

Professor AYRTON: I am inclined to agree with Sir Charles Bright with regard to the difficulties of carrying an over-ground telegraph line through such a country as Central Africa; for it must be remembered that after much experience it has been decided in Germany, although a flat country, and one therefore especially suitable for aerial lines, to employ no more overhead lines, but instead underground wires, consequently one cannot help feeling that a submarine cable is the most suitable means of connecting Europe telegraphically with the Cape.

Mr. Latimer Clark has referred to some insulators fitted with lighting protectors which he sent out to India. In these insulators, which were used for terminal poles there was attached to the iron stalk by means of an arm a long screw, the point of which could be brought extremely near to, but without touching, the iron hood; and this arrangement was, I understand, found to add considerably to the safety of the instruments. Of course such insulators could not be employed along the line as dirt would collect between the iron hood and the point of the screw attached to the stalk, but this would hardly constitute a disadvantage for a terminal insulator as the dirt, should any collect, could soon be seen and removed.

Dr. SIEMENS: With regard to the question whether a land-line or submarine cable would be most to be depended upon, I may be able to speak with some practical experience. I agree with Mr. Preece that the difficulty of carrying land-lines through comparatively wild countries is not so great as many imagine; and our President has had as much experience as any one here in that description of land-lines. We know that a land-line from this country to India, which passes through continents which are in a more or less civilized condition, answers extremely well, and the same may be said of other lines carried through comparatively uncivilized countries, therefore I do not believe the carrying of a land-line through Africa would be a matter of insuperable difficulty.

The question of transport can be met by judicious arrangement. It is necessary to organise the land transport in such a way that those engaged in it should undertake certain possible distances, and

the materials must be so prepared that the pieces are not too much for ordinary beasts of burden to carry. With judicious arrangement this can be met; nor do I believe the natives would, after having been once reconciled to the idea of the line through their country, offer any insuperable difficulties. At the same time I am far from believing the Government have done wrong in aiding by their powerful means the construction of a submarine line. It can be much more quickly established: and when established, offers a reasonable guarantee of permanence. For my own part, had I been consulted by the Government on the subject, I would have been of opinion that the better route for the line would have been not to have passed the coast of Africa, but carried to the opposite coast of Mauritius to the southern part of Egypt. I believe if that had been done the line would have been about equal in length to that to Aden, and would perhaps have given a greater guarantee of safety on the one hand and of more useful results on the other. If a submarine line is carried along the coast, it is beset with dangers which a line passing from one continent to another, or from one part of the world to another is free from. We all know on a long shore the rivers make large deposits of alluvial matter which at intervals of years, perhaps of months, take a slip, and thus it arises that lines laid inshore, even at a distance of 30 or 40 miles from land, are exposed to sudden interruption. Then again it would be very wrong policy to lay a cable in the tropics in shallow water.

Mr. Preece has pointed out the many dangers that beset submarine cables in shallow water in the tropics, which swarms with animal life, and where chemical action is rife; and if these dangers can be avoided by following a course as near along the middle of the channel as may be, it is well, I think, to do so.

Professor Ayrton has alluded to the change that has taken place in Germany from overground lines back to underground lines. I believe that is a very wise change, and one that will be followed in other countries. An underground line is in the safest possible condition against the effects of atmospheric changes and storms, and although we have to contend against one enemy which we do not meet with in the case of suspended wires—viz., induction—yet *this is a constant* enemy, and one which we can, after we have once

dealt with it, silence for ever. This question of retardation by lateral induction is a difficulty which is now entirely removed as an impediment to rapid working. At the same time, while I am strongly in favour of underground land-lines in countries where they have a great deal of telegraphic communication, I should not recommend underground wires in constructing a line through Central Africa, because it would be more expensive, and it could not be so easily inspected. The natives, no doubt, would have greater difficulty in discovering the position of the wires, but still I think there would be great difficulty in screening from them the position of such underground wires, and once they found that by digging they could get out the copper wire, they would be more likely to search for it than they would to take the suspended iron wire, which would at once tell the tale against themselves and lead to their punishment; but I think that what Professor Ayrton has mentioned may be an argument in favour of a submarine cable. So that I believe both land-lines and submarine-lines have each their proper sphere of action, and that both will ultimately be necessary for the communication between our colonies and this country.

The PRESIDENT: I regret very much, for my own sake, though not for yours, because my place was so ably filled, that I could not be present—having been called away on official business—when Mr. Sivewright's paper was read. I have since had an opportunity of seeing the paper, but I have not seen the remarks of those gentlemen who opened the discussion; so that, though I will not keep you long, I fear that I may possibly repeat some of the remarks that may have already been made.

As to the commencement of Mr. Sivewright's paper, I will simply say, from my little experience in countries not entirely dissimilar in some respects to South Africa, that in selecting the line the telegraph should follow, I hold that the wire ought to be kept about 100 yards from the main road—never very far from it. The line of wires in a country like this ought always to be visible to a man riding along the road. With regard to the question of iron *versus* wood for posts, my experience is strongly in favour of iron. Mr. Sivewright alludes to several kinds of wood which he recom-

mends for use in South Africa; of those it is impossible for me to speak, but in India there are endless varieties to be had, none of which, however, for this purpose are to be compared to iron, and none is used on the international lines of communication. Generally speaking wooden posts cost more in the long run than iron. In Persia, where I had a line to construct in 1862-65, we tried all the kinds of wood procurable—and they are not many. We tried poplar; we tried plane-tree (chenàr); and we tried oak, which is more durable, but even that did not last. I have seen a line with straight poles, apparently perfectly sound, and yet a slight shock would upset the whole, and the least gust of wind would bring miles and miles down after two or three years' erection, owing to rot or the ravages of white ants. Iron posts have been substituted in all cases. I have not at present a wooden post in all Persia, and I believe the same may be said of all the main Indian lines. Wood is cheaper at first, but it soon decays. Taking the heavy sneezewood posts weighing 225 lbs., with carriage at £52, 10s. per ton, for 450 miles iron would, I imagine, be even cheaper at first and assuredly more durable in the end. With regard to animals rubbing against the poles, there are not many wild animals in Persia, but there are camels who will scratch themselves when they get a chance. Droves of camels graze along the high roads; and a camel is by no means a light animal. We find no difficulty in keeping them away from the posts, either by building a rubble wall of stone, two or three feet high, round the posts, or where stones are not at hand by surrounding the poles with cut thorns, or by digging a ring ditch three or four feet from the posts all round. These will effectually prevent camels and, I presume, wild animals from approaching the poles, and they are of course avoided by waggons.

With regard to insulators, all I could say has been said already, except that in a country like Persia, where wilful damage is to be apprehended, we are obliged to use iron hoods. Generally speaking I should prefer to use plain porcelain.

As to the main question, although Mr. Sivewright has only devoted a few paragraphs at the end of his paper to it, yet it appears to have attracted attention more than any other point here

raised. I refer to the communication between England and the Cape, and I would say one or two words upon that.

The question of the practicability of the overland route is not, I think, a purely telegraphic question at all. Its solution depends entirely as it seems to me, on geographical conditions, and the consideration of the character of the tribes and people through whose country the overland line would be made. We all know the way in which a question of this kind is discussed. It is spoken of by people according to the particular light in which they happen to look at it. I have never been in South Africa myself, or in Africa at all, and I do not offer an opinion without great hesitation. But the question has been brought before me on previous occasions, and I have had the opportunity of reading the opinions of people who ought to know something about the place. Mr. Mayes, who spoke at the last meeting, and with whom I had the pleasure of working in the far East, is very much opposed to a land-line in Central Africa. He says a cross-country line there is impracticable. I understand that Sir Samuel Baker is of the same opinion. On the other hand Commander Cameron writes that as far as the country itself goes he sees no difficulty in carrying a line through Africa. Mr. Stanley was at first opposed to the idea, but he has since then come to the opinion that a cross-country line from north to south is by no means impossible. "Impossible," is a word we ought to dismiss from our vocabulary, the question is, whether such an undertaking would be worth the cost it would entail? I perfectly well remember being told in 1865 it would be absolutely impossible to maintain a land line to India viâ Teheran. For years I was so disgusted by the wilful injuries done that I was half inclined to think the line would never answer. But after long-continued worry and vexation we have managed to keep the line going, and now wilful injury is comparatively rare. Our line through Teheran to Bushire, and then by cable to India is so well worked that Mr. Sivewright refers to it as a model land-line. I do not say breaks do not occur, but they are rapidly repaired, and the thing said to be impossible is an accomplished fact.

Sir Charles Bright has alluded to the circumstance that when the Persian Gulf cable was laid to Fâo in 1863 the land-line be-

tween that place and Bagdad was insecure, but that had been foreseen, and an alternative line was provided.

Judging from past experience I would only say a line across Africa will cost a vast amount of money, and will entail much trouble, and, of course, great perseverance will be required to carry it out. Probably a cable is best for the present, but I believe that in time to come there will be a land-line across Central Africa. The explorations of Grant, Cameron, Stanley, and others have put us in possession of a pretty good knowledge of the African continent, and although the thing may be difficult and cost a great deal of money, the idea should not be scouted as impracticable.

I repeat it is mainly a question of the character and goodwill of the people through whose districts the line passes. With regard to the line from Calcutta to Rangoon the tropical vegetation is as rank as it well can be, I cannot imagine vegetation more luxuriant, yet through that country the telegraph is kept constantly going. Of course the maintenance of the land-line will cost more than that of a cable. The land-lines in India have never been carried any long distance round simply with the view of avoiding jungles: and looking at the country through which this African line would pass the marsh and jungle land is limited, and the forests are not so extensive as one might at first suppose.

Dr. Siemens has mentioned the subject of laying the cable to South India. On that point I have my own opinion, and I have expressed it already. I venture to think that a cable from Natal viâ Mauritius to South India would be preferable to one laid direct to Aden. It would benefit the traffic to China, Australia, and the far East, and, by having four lines instead of two to Europe, the communication with home would be on a sounder basis.

Leaving the subject now, we have had an interesting paper from Professor Hughes on "The Means of Preventing Induction in Lateral Wires," which has been announced for discussion this evening, and I shall be glad if gentlemen will favour us with their views on the points raised. A paper on a subject much akin to that of Professor Hughes's has also been announced for this evening, and perhaps it is not too late for it to be read.

Mr. W. H. PREECE: I think my paper had better be taken as part of the discussion on that of Professor Hughes's. In the year 1831 Faraday discovered the existence of that which is called by some current induction, and by others electro-magnetic induction. He found when a wire was conveying currents that another wire in its immediate neighbourhood was at the same moment also conveying currents, the latter being induced by the former. In 1848 some long underground lines were laid in Germany, and in 1852 similar lines were also laid in England, and it was found that these wires were subject to a species of induction—not quite the same induction as that discovered by Faraday—called electro-static induction. It was found that these wires were of the nature of a Leyden jar, time being occupied in charging the jar and time being occupied in discharging it. These two inductions—electro-magnetic and electro-static—are those which we meet with in telegraphs, and it is essential that the difference between the two should be clearly borne in mind.

Now, electro-magnetic induction is due to the formation of a magnetic field in the neighbourhood of a wire, and it is purely the result of magnetism. Electro-static induction is due to the formation of an electric field, and is a purely electrical effect. The currents due to electro-magnetic induction are sudden; the currents due to electro-static induction are prolonged. The effects of electro-magnetic induction are what we may call disturbances—breaking up a dash into dots; the effect of electro-static induction is to produce retardation of signals, and to reduce the speed of working of the wires. The direction of the electro-magnetic current at the receiving end is always reverse to the primary current; the direction of the electro-static current is always in the same direction.

Now, in 1875, Mr. Culley brought a paper before this Society in which this matter was very carefully examined, and it took us from the year 1845 to the year 1875 to satisfy ourselves that this current induction exists in the wires. In that year, by a series of careful observations, Mr. Marson proved on two wires from London to Holyhead that disturbances arose from the existence of currents of induction. In India, where the lines are long, this had been

noticed before; in Persia, where the atmosphere is dry and everything favourable to good working, it has also been observed. In Australia they have been troubled a good deal from this cause. This disturbing element was proved, by a series of careful observations by Messrs. Culley and myself, to be due not to electro-magnetic induction, but entirely to electro-static induction. It has been shown where two wires run side by side, where one wire forms the outside coating of a Leyden jar the effect is retardation. The most careful experiments were made on long underground lines, and on coiled cables. There was a cable in 1858 lying in the East and West India Docks which Sir Charles Wheatstone and others experimented upon, and no trace of electro-magnetic influence between wire and wire could be discovered: in fact, until the existence of the telephone, although we had no doubt these currents were there, we had no absolute proof of their existence. The telephone placed in our hands an instrument of great and marvellous delicacy: although it has not to any great extent succeeded as a practical instrument in England, yet it is a most valuable instrument in the hands of the physicist. The most interesting experiments were made through the means of the Post Office by myself, Mr. Marson, Mr. Willmot, Mr. Kemp, and others. I went to Manchester and experimented upon the line between Manchester and Liverpool, and also upon the cable between Holyhead and Dublin. I will not take up your time by going into details, but I will mention generally the effects of the experiments were to show that there was induction between wire and wire, and that this induction could be prevented by two methods. First of all they were considerably diminished by the insertion of a screen of metal between them. We wrapped the wires round with leadfoil and tinfoil. We had a cable—a seven-wire cable half a mile in length—each wire coated with lead. This cable was drawn into one of our London street pipes through which strong currents are passed, and the result was to show that the coating of lead was entirely successful in removing the effects of electro-static induction, and if we could afford the expense of coating all our wires with lead sheathing then the effects of static induction would be entirely removed.

This differs considerably from the results which Professor

Hughes has found out, but while Professor Hughes has only experimented upon a length of a few inches, our experiments were made upon considerable portions of a mile. A good deal has been said about the effect of return wires in eradicating the effects of induction. No doubt the use of a return wire is very effective in diminishing induction, because the one wire neutralises what happens in the other, and the very first telephone I put up was provided with a return wire, and the telephone put up to the offices of the Telephone Company in London had a return wire, and I was astonished to find some months afterwards Professor Bell took out a patent for the use of the return wire. Dr. Muirhead worked in this field, and his use of the return wire fully confirmed the previous effects. But the question of the influence of currents in one wire upon currents in another wire has gained fresh interest to us from the intention evinced to extend electric lighting to different towns in the country. I think the day is far distant when small towns will be lighted by electricity. Nevertheless there are bills before Parliament, which may be brought before Committees of the House, in which power is sought to construct lines carrying wires for the purposes of electric lighting. It struck me at once, inasmuch as the telephone has convinced us of the presence of currents of induction, if we increased the strength of these currents to the enormous extent required for electric lighting, disturbance must arise. The current required for electric lighting is 3,000 times stronger than that used to work the telegraphs, and consequently a mile of wire used to convey the currents for the electric light will have as much influence upon our wires as a line 3,000 miles in length. In order to solve this question I went to the Thames Embankment, along the subway of which the wires are conveyed to the electric lights there.

The result of these experiments has been to show the inductive influences of electric light currents are very powerful. Wherever we used an iron-sheathed cable the effect disappeared. In this case the iron sheath is used as the return wire, and therefore one would imagine the wires laid in iron pipes along the streets the effect of the iron would be to screen the inductive influence between one pipe and another. To solve that question,

we had along Aldersgate Street three lines of iron pipes. One pipe contained 60 wires, another contained 14 wires, and the third contained 30 wires. The pipe containing 30 wires was out of use, the others were in working order. On applying the telephone to those wires, we found that the iron pipe did not act as a screen; that the effect of induction from the working wires was as strong apparently as though no iron pipe existed, and it shows when you have an iron sheath immediately surrounding the line it acts practically as a return wire, but if you increase the sheath you do not destroy the lines of magnetic force, and do not destroy magnetic induction. The result has been to convince us that if we are to maintain good order for our lines we must forbid pipes laid to carry the current necessary for electric lighting, coming within six feet of our own working pipes; and should the question come before the House of Commons it is our intention to oppose every bill unless the promoters agree to a clause by which they give us control over the position of their pipes.

Now, with regard to Prof. Hughes's paper, I have endeavoured to draw the distinction between electro-magnetic induction and electro-static induction. Electro-magnetic induction, as I have said, produces disturbance such as is produced by electro-light currents. Electro-static induction produces retardation of signals and diminished speed of working. Up to the present we have experienced no trouble on our working wires from electro-magnetic induction. I do not think it exists to so great an extent on long wires as on short wires, whilst the effects of electro-static induction are felt more upon long wires than upon short.

I think Prof. Hughes in his paper has cured the first, but he has not attacked the second. His efforts have been in the direction of neutralising the effects of electro-magnetic induction and not those of electro-static induction. He has introduced a principle of some value as regards the working of the telephone, but for telegraph circuits I do not think it will be found of much practical use. Other systems somewhat analogous to this have been introduced. One is that introduced by Mr. Wilson, of Chicago, for neutralising the effects of induction on land lines, and is, I think, *as regards* electro-static induction, better adapted for the purpose

than that of Prof. Hughes. I will not detain you longer. I am sorry I have not had time to write a paper, but I hope I have said enough to show what the results of the experiments have been, and no doubt others may have more to say on this matter if they have considered it. By the time the Proceedings come out I may have an opportunity of putting before you in a better form the results of my experiments, which have been to show if one wire is protected by an iron sheath, used as a return wire, all internal magnetic induction is entirely shut off, but in the case of working through iron pipes it is a different thing. There, it is true, we have the conductor enclosed in an iron sheath, but when you take an iron pipe of three or four inches diameter and you have this full of wires you destroy the symmetry that exists in the former case.

Professor AYRTON: The remarks made by Mr. Preece are of great interest, because they are not merely theoretical, but like the paper itself of Prof. Hughes they detail the results of actual experiments. In the early days of the telephone, when the difficulties arising from electro-magnetic induction began to be serious, there were people who thought that because a metallic screen in connection with the earth will shield a wire from electro-static induction, therefore that if a wire be surrounded with an iron screen it will be shielded from electro magnetic induction, a result quite contrary to the results of experiment, unless, indeed, the iron screen be extremely thick. But if a *return* circuit be employed of such a kind that the magnetic field produced by the current in the one wire is exactly equal and opposite to the field produced by the current in the return circuit there cannot be any electro-magnetic induction. To obtain such an arrangement the return circuit must be as well insulated from the ground as the line wire, a condition practically never fulfilled in sheathed cables. If, therefore, Mr. Preece found that using the iron sheath of his cable as a return wire prevented electro-magnetic induction, I am inclined to think it must have been because the iron sheathing was well insulated from the ground, and that any other equally well insulated sheathing of any other metal would have answered just as well, and, in fact, if he had insulated from the ground the

iron pipes containing his wires and used each iron pipe as the return circuit for the wires inside it, that no practical electro-magnetic induction would have been experienced from the wires inside one iron pipe on those inside another. As, however, the iron pipes under the street never are insulated, they cannot, I apprehend, be used practically to prevent electro-magnetic induction from one wire inside one pipe to one wire inside another, no matter whether the wires were arranged symmetrically inside their respective pipes or not.

I am inclined to think Mr. Preece, in his great interest for the welfare of the Post Office Telegraph, is proposing an unnecessary legislative restriction in fixing a considerable space as the minimum distance between the subterranean wires conveying the electric currents for the electric lights and the wires used by the Post Office, seeing that if only it be stipulated that the Electric Lighting Companies use as their return wire a metallic tube of *any metal surrounding* the wire, *insulated from it along its length and from the earth*, all practical electro-magnetic induction will be prevented, and no impediments will be placed in the way of the Electric Lighting Companies.

Professor HUGHES: I quite agree with Professor Ayrton as to the fact that an iron sheath would be no protection whatever. I have demonstrated this fully in my paper, and I feel convinced that if such investigation had been first made, there would never have been advanced the theory that a small tube of iron would be a complete protection against lateral induction.

Mr. Preece, after having himself proposed several remedies for induction, now declares either that it does not exist or that it is so small as to be entirely negligable. He fears, however, the influence of the wires conveying the current of the electric light, as that kind of electricity, according to Mr. Preece, is not possessed of the peculiar benevolent attributes of the special kind of electricity used by the Post Office. It would be interesting and highly useful if Mr. Preece would be kind enough to inform the Continental Governments of this peculiar electricity which possesses no inductive powers.

Since the reading of my paper Mr. Edison of New York has

telegraphed to London that he had imagined a very similar instrument to the one shown, and that his claims would be found in Patent No. 2909, 1877, on telephones. Had I been aware of this patent, I should have freely acknowledged his claims as set forth in his patent, and in a letter to the public journals this week I have at once acknowledged this claim, and showed the very important differences which exist between the instrument I have shown and the one patented by Mr. Edison.

In describing my instrument in my paper I first described the lines with coils at relative distances, and showed how this would just double the induction and do more harm than good. Now this described exactly the theoretical conditions as shown in Edison's patent; but after showing what it would not do, I demonstrated how, by fulfilling certain conditions—not one of which are mentioned in Mr. Edison's patent—a perfect induction balance would be obtained for all lines. These conditions are too long to repeat, having already been stated in my paper, but they may be summarised thus:—1st. Coils must be reversed at instant of putting on battery. 2nd. Coils must all have a certain definite length of wire in exact proportion to that of the line used upon. 3rd. Battery power must also be proportionate to length, so as to have equal force on each line.

There are many minor details, such as provision for extra currents, static charge, &c. The main difference, however, between mine and Mr. Edison's is—leaving aside the question of practical value and utility—that a simple laboratory experiment, as described in the English and American patents, will compensate for a single line, but not for the rest of the many adjacent wires.

Mr. Chas. H. Wilson's very beautiful and superior arrangement will perfectly compensate for two lines. The arrangement I proposed will compensate for all lines, each adjacent wire being perfectly free from induction. As public notice was given that this discussion would take place this evening, I was in hopes that Mr. Edison's agents and representatives in London would have been present to-night, to fully explain Mr. Edison's researches; but as they have remained silent, I deem it unnecessary to take up more of your valuable time on this subject. Thanking the President and

the audience for their kind attention, I will now close my remarks by inviting those present to inspect the instrument in question in practical operation at the close of this meeting.

The following Candidates were then balloted for and declared to be duly elected:—

As Foreign Member.

Enrique de la Precilla.

As Member.

Edward Alfred Cowper.

As Associates.

Philip Cardew, Lieut., R.E.
George Keith Gordon, Lieut., R.N.
Cooper Penrose, Lieut., R.E.

The Meeting then adjourned until April 23rd.

The Seventy-eighth Ordinary General Meeting of the Society was held on Wednesday evening, April 23rd, 1879, at the Institution of Civil Engineers—the PRESIDENT, Lieut.-Colonel BATEMAN-CHAMPAIN, in the Chair.

The proceedings commenced by the reading of the following paper:—

RECENT IMPROVEMENTS IN PROFESSOR BELL'S TELEPHONE.

By ADAM SCOTT.

Ever since Professor Bell introduced to this Society his invention of the Articulating Electric Telephone, the interest felt by the members in that instrument, and in the numerous discoveries that have followed in its wake, has been so strongly shewn that I have felt it not out of place to draw your attention to the recent improvement made by Mr. Frederick Allen Gower in the instrument commonly called the Bell Telephone. I do so all the more readily, as I believe that since Professor Bell lectured in this room, about eighteen months ago, no further communication as to the construction of that telephone has been read to the Society, and it might therefore seem to have been stationary all this time.

When I speak of the instruments commonly known as Bell Telephones, it will, no doubt, be understood that I refer to those in which permanent magnets are used. At the same time, it would not be right to forget that the first Telephone he invented was an Electro-magnet or Battery Telephone, and that this has been the parent of several forms of instruments introduced by subsequent investigators. As, however, the Professor selected the Permanent Magnet Telephone for circulation, on account of its greater simplicity rendering it more suitable for general use, it is perhaps not to be wondered at that the public have associated it more specially with the inventor's name.

In America the Bell Telephone has had an almost unparalleled success. The proprietors do not sell any instruments, but simply

let them out on hire at an annual rental. Nothwithstanding this seemingly disadvantageous practice, I understand that the present number of telephones thus let on hire is in excess of 26,000, which is an enormous number considering the comparatively short time since the invention was introduced. And the business seems to be going on progressively, the further issues being now about 2,000 monthly. In all the large cities of the United States the Bell Telephone Company have established the central office or exchange system, by means of which any one subscriber can, at almost a moment's notice, be put into communication with any other subscriber. In Chicago, for instance, the number of subscribers is over 700, and so much are the facilities for inter-communication appreciated and availed of, that, as I learn by last advices, the "calls" through the telephone upon the central office number over 8,000 daily.

These figures show clearly that our American brethren—perhaps on account of their admitted spirit of enterprise, or go-aheadedness—are much more appreciative than we are of the benefits to be obtained from the telephone. Perhaps, also, it may be that there has been much better management of the business in America than in this country. Whatever be the cause, the fact remains that the telephone has not yet been adopted in the United Kingdom to any appreciable extent. I do not hesitate to say that there are not anything like as many as 500 telephones in actual daily use, as compared with the 26,000 on the other side of the Atlantic.

This, in some respects, is to be attributed to ignorance on the part of the great majority of the public. Notwithstanding that the telephone and its wonders have perhaps been the subject of more newspaper paragraphs than any other invention, there is no question I have had so frequently put to me by well educated people than "Do you think it will ever be a practical instrument?" or "Will it work more than a few hundred feet?" Yet most people say they have heard or seen the telephone; it seems to me, therefore, that this melancholy state of the public mind is due to the fatal facility with which telephones of a kind can be made. When people first got over their unbelief at the strange Yankee

rumours that reached our shores, they eagerly read the descriptions given of the wonderfully simple instrument, and if they did not set to work to construct it for themselves, they purchased it from the numerous pirates that sought to make a profit out of the public curiosity, regardless of the quality of the instruments thus sold. Those who know with what care and exactness in every respect a telephone requires to be made in order to give satisfaction, can easily understand how the public have arrived at the opinion that it is a pretty scientific toy, but little else. The proprietors of the telephone in England are not altogether to be absolved from blame in this matter—the instruments they have supplied to the public, although infinitely superior to those just referred to, were not such as should have been issued. In the hands of non-electricians, the faults of their manufacture led to the instruments becoming frequently silent, and to their requiring such constant attention on the part of skilled persons, that they often became a nuisance instead of a convenience. This suicidal policy has, I regret to say, been continued until lately, with the result that the telephone has, through mismanagement, failed to make that progress in general estimation, and in practical application, that its intrinsic merits entitled it to.

In the United States this error was not committed. It was early seen that for practical purposes the first form of the instrument must be much improved. More than a year ago the wooden hand telephone was abandoned, and greatly superior instruments substituted. For the purpose of comparison I have laid on the table specimens both of the wooden telephones issued in this country, and of the American ones that have been so justly successful.

There is no necessity in a meeting like this to explain the principles and action of the Bell Telephone. It may be useful, however, to explain somewhat in detail, what I may fairly call the disadvantages under which it laboured in this country.

The most noticeable drawback to its use has been the comparative faintness of the sounds produced. While the articulation was very distinct, the volume was small. As a consequence, it was simply impossible *to use* the instrument with beneficial results in

places where the utmost quiet did not prevail. As this necessary quietude is not a condition easily attainable in the majority of business offices, it followed that the field for the utilisation of the telephone was thereby much narrowed. Even in places ordinarily quiet, the sound of another person talking in the same room was sufficient to prevent the listener hearing well; and under the most favourable circumstances there was so much strain upon the attention required that communication through the telephone was apt to become somewhat fatiguing.

This want of volume was primarily caused by the smallness of the magnets used, and their extreme weakness. It seems to have been thought that any penny magnet was sufficient for the purpose of generating those currents, whose very weakness was adduced as one of the marvels of the instrument. That no greater mistake could possibly have been made will, I think, be evident later on when I shall have to show that the stronger the magnet the better the result that may be expected from it. The choice of small magnets was perhaps dictated by a desire to produce a telephone of convenient size and weight that could, without much weariness, be held to the ear for some time if necessary. The combination of a fixed telephone, with a powerful magnet and a flexible speaking tube, does not seem to have suggested itself. Such a combination would have got rid of the disadvantage arising from the faintness of the sounds produced, and besides would have done away with the annoyance caused by the use of flexible wire conductor cords, which are a necessity with hand telephones. The wires in these cords, especially where not very tenderly handled, are constantly breaking, and thus interrupting the communication, without there being any external indication of the cause to enlighten the the uninitiated. Such broken wires often momentarily come together again, and thus communication will be temporarily restored, to cease perhaps in the middle of a sentence, as the movement of the user causes the broken ends again to separate.

These breakages not only cause annoyance, but they occasion much trouble, as nine times out of ten they necessitate the sending for some one who understands the cause, and the waiting until he can come with new cords to replace the defective ones.

Another complaint against the telephone (I speak of experience in this country only, because it is not the case in America) owes its origin also to the weakness of the magnets. In order that the maximum effect shall be obtained, it is necessary to adjust the diaphragm exceedingly close to the pole of the magnet. The result is, that in I am afraid the majority of instances, any alteration of temperature, or of moisture affecting the wooden cases, causes the diaphragm to touch the magnet. Once touching, it is sure to remain in that position, and, being no longer free to vibrate, fails to produce sufficiently audible sounds. This, however, is a defect in manufacture that ought not to occur, but it is well to point it out, as it is only by knowing what such defects are and their causes that we can find a remedy.

There is another disadvantage that the telephone hitherto has had to contend against. The sound produced being so faint, it was necessary to draw attention to it by means of separate apparatus. Magneto Bell-Calls are largely used in America, and were introduced into this country, but for our credit I regret to say that in almost every case (and I know of numerous ones) these Magneto Bell-Calls were a signal failure. Why they have been a success in the States, but a failure here, can only be set down to bad manufacture. Battery Bell-Calls, therefore, became a necessary adjunct to the telephone, thus adding a very undesirable complication to what was otherwise so simple, not to speak of the extra cost they entail. Bells and Batteries for a short line cost at least £4, which is large as compared with the actual cost of the telephones themselves. Such calls necessitate many connections, contacts, and switches—all of which increase the chance of something going wrong—and are so complicated and technical in nature, that both in erection and maintenance the intervention of a skilled person is absolutely required, and until such assistance is obtained (very often from a distance) the apparatus in the event of mischance is useless.

I have alluded to the fact that some of the disadvantages I have described as hitherto attaching to the telephone in this country are not common to those used in America. The two kinds of instruments used in the States, and which are laid upon the

table, are certainly great improvements upon the original telephone. They are very superior and reliable instruments. As, however, they have been largely in use for more than a year, they are not exactly recent improvements. I will not, therefore, take up time in dwelling upon them.

The particular telephone which I have the pleasure of introducing to the notice of the Society this evening is the result of recent improvements devised by Mr. F. A. Gower. It will be remembered that this gentleman, about a year ago, exhibited his telephonic harp to the Society. He was formerly associated with Professor Bell and the American Telephone Company from the early days of the invention, and is now engaged in the telephone business in France. Mr. Gower has for several months devoted himself exclusively to the study of the various means by which the disadvantages I have described could be obviated, and to the improvement of the instrument in every detail. I am glad to bear testimony to the perseverance and ability with which Mr. Gower has pursued his investigations, and of congratulating him upon his success, of which I hope the Society will be able to judge for themselves.

Mr. Gower has produced a telephone so powerful that I have no doubt music, and even in some degree, spoken words will be audible all over this room. When it is provided with a funnel for collecting sound, as in the case of the instrument on the table which we shall use this evening, conversation can be carried on without speaking or listening closely at the telephone. And so sensitive is it that all sounds made in the room, however large in which it may be, are distinctly audible at the other end of the line. I have no doubt that whatever I may say at as great a distance as I can get from the instrument will be heard by my friend at the other end, and, I hope, so distinctly as to be repeated by him.

Such experiments are no doubt interesting in a lecture room as showing the delicacy and sensitiveness of the telephone, but for practical purposes they count for little. What is wanted is that combination of volume with distinctness that will make the instrument useful and reliable not only in a busy office, but in

factories amidst the working of machinery, and on the field in military operations. A telephone that speaks so loudly as to be heard over a room is for most purposes not only not necessary, but has its disadvantages, by depriving the communication of that private nature which is a desideratum, especially as the sender of the message cannot possibly tell to whom and to how many people he might thus be unintentionally talking.

When we come to examine the instrument as shown upon the table, and also illustrated by the annexed diagrams, the first thing

that strikes one is, that after all it is a simple Bell Telephone, composed of a diaphragm and horse-shoe magnet, produced with two soft iron poles on which are insulated wire coils. Wherein, then, it may be asked, lies its peculiar excellence? and the answer is, in the perfection of each part, and in the combination as a whole. When our attention is drawn to the fact we will find that in every detail a different form is used to any which we see in ordinary telephones. Let us therefore proceed to an examination of each part, beginning with the magnet (O). We see that the ordinary

form of the horse-shoe magnet has been abandoned, for one of an entirely novel shape, an arc of a circle, with its chord; the division between the poles being in the centre of the latter. The magnet is made out of a peculiar kind of French steel, very porous in its nature, and has been magnetised in a special manner, which is at present the only secret of the manufacture. The result of the combination is a permanent magnet of great power, capable of lifting at least ten times its own weight. This strength is a most important feature, and has not been arrived at except after much patient investigation and a multitude of experiments. There are very few people here who could by a fair outward pull pluck the armature from the magnet I hold in my hand without very great difficulty, and I need scarcely point out the striking difference between such magnets, and those formerly used in the construction of telephones.

The magnet is provided with soft iron pole pieces, but the usual cylindrical shape has been abandoned, and instead we have poles which present to the diaphragm a narrow oblong surface, as long as the width of the magnet, and about one-eighth of an inch in breadth. The coils follow the shape of the poles, and are each of about 60 ohms resistance. This peculiar form allows the poles to be brought much closer together than in other telephones, and they certainly attract the diaphragm more strongly than in ordinary telephones. The coils, as a mass, are also much more within the most powerful part of the magnetic field, an advantage that must be obvious to all.

The diaphragm, which is $3\frac{3}{4}''$ in diameter, is much thicker than usual, so much so that, notwithstanding the great strength of the magnet, a very close adjustment near to the poles is practicable without any chance of contact, and the closer the adjustment the more sensitive does the instrument become. The diaphragm is very firmly held by a brass ring screwed into the cover of the telephone.

The whole apparatus is enclosed in a metal box, partly for the purpose of strength, partly to avoid the increased vibration and consequent reverberation that accompanies the use of wooden boxes, and partly because there is less alteration from expansion and con-

traction, and moisture cannot be absorbed. To the cover of the box a flexible speaking tube is attached.

I have now to draw your attention to one of the most important features of the instrument, and one which is entirely the invention of Mr. Gower. The telephone gives its own call, a call sufficient in most cases to obviate the necessity, the expense, and the complication of electric bells and batteries. In the diaphragm a small hole is cut, and to the reverse side of this is soldered a small box or trough (A) containing a metal tongue or reed (T). When the user wishes to call attention at the other end of the line he does exactly as he would do with an ordinary speaking tube, with the use of which every one is familiar. He blows into the flexible tube, and the air passing through the orifice in the diaphragm vibrates the reed, and then escapes by a vent made in the side of the box. The sound produced is very distinct and pronounced, and the diaphragm is put in intense vibration, due not only to that sound, but to the mechanical communication of the vibration of the reed caused by its being attached to the diaphragm. At the receiving end the call is very satisfactory, and of this the meeting will be able to judge for themselves.

Mr. Gower's telephone is not the only one which contains its own call, though decidedly the simplest and best. Mr. Siemens, of Berlin, uses a reed temporarily held in the mouthpiece of the telephone, by which not only is the sound produced by the vibration used, but it is made to act on a small ball which produces a series of blows upon the diaphragm or disc, the result being an audible signal at the further end of the line. This invention requires the telephone when not in use to be kept in an upright position.

Another method of making the instrument make its own call has been patented by Mr. A. F. St. George, of the India Rubber Company. Outside the coil of the telephone another coil of thick wire is wound, an intermittent current from a battery being passed through this outer primary coil, corresponding currents are induced in the inner coil, which as usual is permanently connected with the line, and cause vibrations of the diaphragm at the distant telephone audible at a considerable distance. The vibrations are

rendered intermittent by means of an armature vibrated by an electro-magnet upon contact. The switching in and out of circuit of a bell-call is thus obviated.

In concluding, let me call attention to the great simplicity of the instrument. It consists of a magnet, coils, diaphragm, reed, the containing box, and a speaking tube—no batteries, no bells, no switches. That this simplicity is a very important matter will scarcely be disputed, and when added to it one finds that the telephone has great volume and distinctness, that it is strong and cannot well get out of order, contains its own signal and does away with flexible conducting cords, I am sure that the Society will agree with me that a great advance has been made in the department of magneto-telephones, and an instrument been produced that may safely be issued without fear that any person of ordinary intelligence would not be able to fit it up if furnished with very few and simple directions. In these respects it differs from all other telephones yet brought forward, and, I have no doubt, has a good future before it.

The President then invited discussion on the paper.

Mr. W. H. PREECE: As, perhaps, I have taken more interest in the telephone than most men, I may be allowed to commence the discussion by a few remarks. I have been extremely pleased with the performances to-night, and what astonished me more was that there is so little absolutely new in the apparatus, and yet that little new has produced such excellent results. One or two things strike me, in nearly all experiments with the telephone, that is, that every disc of a telephone has its own peculiar dominant note peculiar to each instrument, and when we heard the duet on the cornets, and when they were played singly, it was evident that certain notes gave out sounds louder than others. Those louder sounds agreeing with the dominant tone of the disc, in order to obtain the greatest effect on the speaking telephone it is a good plan to find out that dominant note and speak on that note; and it is because some men have a voice in ordinary conversation which strikes upon that note, that their voices are louder

than others. My assistant, Mr. Wilmot, has a remarkable knack of striking the dominant note of the telephone, and had he been where Mr. Woollaston was, I am certain his voice would have been heard all over the room.

Now, Mr. Scott in his paper has referred to the very extensive use of the telephone in America, and he said at this moment no fewer than 26,000 were paying annual rents, and he also said he did not think there were more than 500 in daily use in England. I think on the latter point he is wrong. Probably the Telephone Company do not reap the benefit from more than 500, but there are others who have nothing to do with the Company, who are using them to a large extent. It is not to the credit of the persons using them, but my impression is that there are at least 25,000 in use in England. This speaks either badly for the morality of the English nation, or it speaks worse still for our patent laws. No doubt the first thing after a patent has been secured is to see if the thing cannot be done in another way, or if not, to study how to avoid payment of the royalty. The telephone has been used in this country to a large extent, but there does not appear to be the want of it in England that there is in America. One thing which strikes one in America is the enormous extent to which they apply the telegraph and the telephone for their own domestic purposes. In Chicago, where there are from 7,000 to 8,000 calls daily, there is scarcely a house which has not in its hall a call-bell, by which you may despatch a message for a doctor, or a porter, or anything else you want, and the reason they are driven to that is—necessity being the mother of invention—that it acts as a substitute for servants. Here we have no difficulty in getting servants if we pay them, but the difficulty in America is to get "buttons" at any price to run about for you as in England, and the result is the absence of servants has to a certain extent compelled the Americans to adopt this system of telegraphy for their own domestic purposes, and the telephone is to be found in almost every house as the only available substitute for the old system.

Few have worked at the telephone much more than I have. I have one in my office, but more for show, as I do not use it because I do not want it. If I want to send a message to another room,

I use a sounder or employ a boy to take it; and I have no doubt that is the case with many others, and that probably is the reason why the telephone has not been more adopted here. The efficiency of the instrument in England has been seriously interfered with by those fearful inductive effects which are not felt to the same extent in America, because they have no long underground lines, and they do not use that fast-speed apparatus which produces such a tremendous roar with us. It is impossible with 60 wires in underground pipes to speak through the telephone, the inductive effects being so great.

The management of the telephone in England has been singularly unfortunate. There has been no policy struck out, there has been no administration to bring the thing before the public in a proper way, and its representatives were not remarkably successful in bringing it before the public, and the result is, it has stopped as much from inefficient management as from inefficient apparatus.

There are one or two things introduced here as new which are not new: for instance, this flexible tube. I have had that in my office for twelve months. The telephone in my office was supplied with a tube of this kind, but though it aided conversation to a certain extent, it induced mixture of sounds or retardation, which interfered with clear articulation, and I noticed that speaking through a tube was not so clear as through the cone. The cone is not quite new, for when I had the pleasure of bringing before you the beautiful discovery of Professor Hughes—the microphone —I used a cone of this description, and every one in the room was able to hear Professor Hughes speak.

The principal feature in this instrument is, first, the form of the magnet, the way in which the poles are placed, and the particular form of the coil pieces. These are to me new. I have seen a good many forms of telephone, but I have not seen that. I think the effect is due, first, to the form of the magnet, and secondly, to its extreme strength. One must be surprised to see a magnet with such portative power as we see here. I don't know that I have ever seen one which would hold more than four or five times its own weight—and practically we are content with

one which holds its own weight—but here we have one which holds ten times its weight, and doubtless to that is due the loudness of the sounds we get.

One thing puzzles me, and that is, why the fixture of this reed in a particular part of this disc does not interfere with the clearness of the articulation. A curious discussion has been going on in the scientific journals upon the theory of this instrument. There are two theories referring to the action of the telephone. The one attributes the emission of sound entirely to the molar motion of the diaphragm—that the vibration backwards and forwards of the mass of the disc produces these sounds. The other theory is that of molecular action—that the sounds are produced by the molecular vibrations of the magnet itself, and these are imparted to the diaphragm. Thus there are two distinct theories—one the molar and the other the molecular. The molar theory has been hitherto accepted in England, and it is by some supposed to be the theory first announced by Professor Bell himself; but when I was in America Professor Bell preferred the molecular theory. The molar theory was first enunciated by myself before the British Association at Plymouth; and I made a great number of experiments to try to satisfy myself that the sound was really produced by the vibration of the disc, and I had no difficulty, when the sounds were loud and by using wind instruments, in feeling the absolute vibration of the disc.

On the other hand the Count du Moncel and many others are violent supporters of the molecular theory. That theory, however, I think, is rather disturbed by what we see here. I should have thought the fixture of this reed would have interfered with its articulation, but I understand Mr. Gower found no difference in the instrument when the reed was taken off it.

The chief merit of this instrument is its simplicity, but I am not sure something has not been lost in endeavouring to get loudness. I do not myself think we want loudness. In an instrument exhibited by Professor Tyndall loudness was carried to excess. The telephone of Edison is based upon the electro-chemical principle, and by the application of that to the disc, sounds were produced loud enough to fill the theatre of the Royal Institution,

but the loudness was obtained at the expense of articulation. Though the articulation was good, it was not so clear as from these little telephones with small discs, and I am inclined to think from what I have heard to-night, the loudness of this telephone is obtained at the expense of clear articulation. When you listen closely you hear what Mr. Woollaston says, but when you go to a distance and try to make out what he says you fail to do so. It is like the phonograph. I have not yet seen a phonograph which faithfully repeats the human voice. And some of you will remember at the Royal Society I challenged the whole body to repeat from the phonograph any part of two sentences which I repeated. I spoke them clearly and distinctly, and yet there was not a man who could repeat a single word. It is a defect when we attempt to get loud sounds, but you do not want to get an instrument to shout out. You want sound loud enough to hear yourself. If you want to send a secret message from your office you don't wish every one to know what is going on. I do not see any practical utility in these loud sounds. I think this endeavour to get loudness is a mistake. What you want is clear articulation. There is no telephone that I know of which produces the letter S; it comes from the instrument as a sort of breath; it fails to transmit the sibilant sounds altogether. Hence in speaking through the telephone absence of clear articulation and want of clearness render repetition so frequent, that those who have used the telephone, regard it more as a scientific toy than as a real practical instrument.

Major WEBBER:—I think the last speaker has left me the opportunity of congratulating Mr. Scott upon having brought before us a most interesting paper, enabling us to discuss a subject which for upwards of 18 months has not come before the Society, and yet which has in the meantime been making a great noise in the world, and also has set many telegraph engineers here and abroad thinking and inventing. I think, whoever is the inventor of the telephone as a means of communicating articulate speech has conferred a boon on society, which I am sorry to say Mr. Preece does not seem quite to concur in. The statistics we have heard of the telephone in America, and what we know is doing in *France* and Germany, show that this means of communicating

articulate speech, and placing persons who wish to speak to each other face to face, is spreading rapidily, and will soon make an immense revolution in the facilities for carrying on business and producing economy in its operations. I cannot help thinking from the few experiments I have made myself, probably in a short time means of communication by articulate speech will be established, and will be in common use in many places by means of a carbon transmitter and a magnetic receiver. In the carbon transmitter an animal tissue diaphragm is used, by means of which I have been able to speak through 70 miles of line with perfect clearness, although it was in a low tone of voice. But this experiment had a condition which could not be got over if it had not been for this animal tissue diaphragm. This 70 miles of line was on poles carrying a number of other wires, and in one place for some distance passed through an ordinary telegraph underground cable containing some 20 or 30 wires, all of which were at work at the time. The difference between the sound of voice when this transmitter was used and the sound when the ordinary magnetic transmitter was used, was this—that in the one the noise from the induction did not interrupt the voice at all, or only to such a trifling extent as not to interfere with it, whereas when the magnetic transmitter was used the sounds produced through the telephone by induction were such that the voice could scarcely be understood.

With regard to the molar and molecular theories, although we have very little evidence with regard to the latter, I do not think the opinions of such a scientist as the Count du Moncel can be so easily set aside as has been suggested, more particularly as in the telephone invented by M. Ader, who submitted it to the Count before he patented it, such marvellous results were produced. In that telephone the principal feature is this, the core is not magnetic at all, but consists of a piece of iron subjected to torsion, strain, or tension. The diaphragm which is used, the same nearly as the ordinary magnetic telephone, produces the sounds. As soon as the torsion or strain is removed the piece of iron ceases to act. This appears to me to favour the molecular as against the molar theory.

There is one matter of interest to telegraph engineers which I would refer to, that is, that the telephone is useful as a means of

helping line men to remove faults on ordinary overhead telegraph lines. I remember on a Parliamentary Committee of the House of Commons on Postal Telegraphs the question was asked why telegraph line men who failed to remove faults should not be able at any time to communicate with the station at the end of the interrupted wire, and thus be able to ask the office whether the fault continued. The answer of the telegraph engineer who was examined was that it was not an arrangement which was likely to succeed. But at the present moment the line men of the Post Office Service, under my superintendence, of their own accord have made a telephone of their own, which they use in a very ingenious way for the removal of faults. They leave a telephone at the faulty end of the line, and as they proceed they attach other telephones to the wire. In this way they communicate with the clerk in the office, and by that means a number of faults are removed by the line men, who might otherwise traverse the line without seeing those faults.

Lord LINDSAY: I want to say a word about Mr. Preece not using this instrument, and my own experience with regard to it. Professor Bell sent me two of these telephones, and since that time they have been daily in use in communicating between my house and my laboratory and observatory, a distance of about a quarter of a mile. I have found them of great use. In the old days I had a telegraph, and there was time lost in calling and then signalling. Now I get my messages delivered in a moment. I know who is at the other end by the sound of the voice. And I find it of great value. As to having large and powerful magnets, I agree with that. I used a compound horse-shoe magnet, about a foot long—a very heavy magnet with coils on the end of the poles of about 45 to 50 ohms resistance—but I found I did not get good articulation, although I obtained an immense collecting power of sound. Talking going on in the laboratory 100 feet from the telephone was audible at my house, a quarter of a mile distant, quite distinctly. Without being able to hear the exact words, I could tell who was speaking in the laboratory.

As to the question of induction: going from the laboratory to the observatory I have a cable of 16 or 18 wires, and another cable communicating with my house of 8 wires. On the 16-wire

cable I have 5 or 6 circuits which are constantly at work in different parts of the observatory. My telegraph circuit going to the house comes within 6 feet of this cable, and I am able at the house to hear the burr at the end of the current at any time, as it is continually going on from the Meidinger cells. I have not found a means of getting rid of this induction. It is said by passing telephone wires through tubes it can be done.

With reference to the articulation of the plate of the telephone shown us this evening being affected by the reed placed on it, I imagine it would not affect articulation, but it would very much change what is called the fundamental note. If this reed is placed in a different position I imagine the vibration of the plate would not be affected except so far as to change the fundamental note; so that if Mr. Preece finds that the fundamental note is altered when this vibratory tongue is removed, I fancy he would have to pitch his voice in a different key to produce the same effect.

Professor AYRTON—Mr. Preece in his remarks has alluded to a very important point in connection with the telephone and phonograph, namely, the difficulty that has been experienced in increasing the volume of the sound without diminishing the clearness of the articulation. This consideration is in reality intimately connected with an important feature that characterises the diaphragm of the telephone and phonograph. It is quite clear that in order that the receiving diaphragm may faithfully reproduce all the vibrations imparted to the sending diaphragm the natural vibrations of each must be unimportant. Now, it is rather interesting that before the invention of the telephone Professor Perry and myself were engaged in the solution of a problem which at first sight might appear to have no connection with telephony, but which really has an important bearing on the subject. We were investigating how to make the best form of seismograph or earthquake recorder. Now a good seismograph must be an instrument which records every vibration, small or great, imparted to it by the trembling of the earth, but which has no preponderating vibrations of its own to interfere with the earthquake record. Although many seismographs had been devised prior to our investigation of the subject *in 1877* no one had been invented fulfilling *this important condition.* Mathematical analysis, however, led us

to this solution of the problem of constructing a proper seismograph.* If we wish to have a moving body which shall faithfully record all vibrations imparted to it, then it is necessary that the natural vibrations of the moving parts must be *very rapid* compared with those which it is required to record, or, if so great a rapidity of natural vibration is practically unobtainable, then the introduction of viscous friction will assist matters in causing the natural vibrations of the recorder to die away quickly so as not to preponderate over those imparted to it, and which it is its province to record.

Now, natural rapidity of vibration is of course attained by rigidity, so that we advocated the use of very stiff rigid springs in our seismograph, the result being that we obtained in miniature a true record of the earth's tremblings. Exactly for the same reason, but in this case as a result of natural selection, Professor Bell and Mr. Edison have been led to the comparatively rigid, naturally rapidly vibrating, membrane of the telephone and phonograph.

The other solution, however, to which we arrived—the introduction of viscous resistance—although used by Sir Wm. Thomson in his dead-beat galvanometer for cable speaking, &c., does not seem to have been tried for telephones or phonographs. But I am inclined to think that possibly, if large diaphragms of considerable rigidity and with perhaps oil-cells to give them viscosity be employed, loud and at the same time clearly articulating telephones and phonographs may be made.

As regards the two theories of the telephone, to which Mr. Preece has referred, there are on the table some rough forms of telephones made at the suggestion of Mr. Latimer Clark on the plan proposed by M. Ader. Now although these telephones have been made as closely as possible in accordance with M. Ader's plan, which is clearly and well described in the Telegraphic Journal, I have not succeeded in getting them to work satisfactorily. At the same time, although such rough experiments as I have made rather point to the motion of the diaphragm as a whole producing the sound in the telephone, I should not care to put them forward as

* "On a Neglected Principle that may be employed in Earthquake Measurements." *Trans.* Asiatic Society of Japan, 1877, vol. v., part 1, p. 181. Reprinted "*Phil. Mag.*," June, 1879.

in any way overturning any of the fairly complete experiments made by the Comte du Moncel, and which show that the action is a molecular one.

Probably the complete explanation is, that the two actions, the motion of the diaphragm as a whole, as well as the rapid molecular extension and contraction of the steel or iron-rod, are both effective in the ordinary receiving telephone in emitting the sound.

Mr. E. A. COWPER: It was stated by Professor Bell that he had a diaphragm $\frac{3}{8}$th thick and 3 feet diameter, and spoke very well with it.

Professor AYRTON: If you have a diaphragm of large size, it is necessary to have it of great stiffness, and therefore thickness, in order that the gravest note naturally emitted by it shall be high compared with the highest note it is ever forced to emit, and which, as I have pointed out, is the condition necessary to be fulfilled in order that its natural vibrations shall not produce confusion.

Mr. ADAM SCOTT: I beg to thank you for the attention you have paid to my paper, and I am glad it has been the groundwork for the remarks that have been made. I have nothing particular to answer, but when people make experiments with the telephone, it is astonishing how different their experience is. Mine differs from Mr. Preece's. I do not find that tubes have the effect of making the sounds less clear. I tried an experiment with three yards of tube, and the articulation was better with three yards than it was with one yard. The volume was weaker, but the articulation was more clear. After what Mr. Preece has said about the subject, I shall make more experiments. In testing the articulation of particular letters, I have noticed there is a distinct difference between B and P, and D and T, but I have not noticed the peculiarity which Mr. Preece mentions with regard to the articulation of the letter S.

The PRESIDENT: I feel sure you have all, like myself, been much interested in the paper Mr. Scott has read, and that you will agree with me in returning him a vote of thanks for his kindness; and also in congratulating him on the remarkable success which has attended the experiments he has made to-night.

The *vote of thanks* having been unanimously accorded to *Mr. Scott, the meeting* adjourned till the 14th of May.

ORIGINAL COMMUNICATIONS.

PADDINGTON, *July* 10*th*, 1879.

MY DEAR SIR,

Having, on looking over some old papers, found a very interesting report from one of my Inspectors of a thunderstorm in which the lightning played a very peculiar part, I send you a copy of the report, which may interest some of our members.

I am, my dear Sir,

Yours faithfully,

C. E. SPAGNOLETTI.

PROF. AYRTON.

REPORT ON A THUNDERSTORM.

On the 23rd July, 1878, I was working with some men in the Clifton Down Tunnel near Bristol, when a very heavy storm, accompanied with thunder and lightning, came on. It began about 1.30 p.m., and about 1.45 p.m. a most fearful flash of lightning struck the metals at the mouth of the tunnel and traversed them into it towards Clifton Down Station (east to west) for a distance, as nearly as I could judge, of 450 to 500 yards; its appearance was something like the flare of a candle. It passed my carpenter about 150 yards, and another of the men about 350 yards in the tunnel. The noise during its course was very similar to that of gun caps or the cracking of sticks in a newly lighted fire. After passing the first man, and before passing the second, the fluid seemed to have divided and jumped backward and forward from rails to wall until it exhausted itself at a distance of about 450 to 500 yards; it did not run up to where I was standing with the other men, but the two whom it passed say that during its passage the air was similar to a whirlwind.

There were several other very vivid flashes seen at the end of the tunnel, but none entered it.

(Signed) T. COCKELL, Inspector,

Chippenham.

REMARKABLE PHOSPHORESCENCE IN THE PERSIAN GULF.

To the Secretary of the
Society of Telegraph Engineers:

KURRACHEE, 12*th August*, 1879.

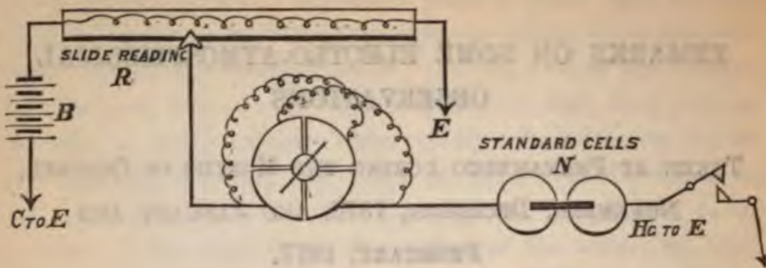
SIR,

The following account of an electrical or phosphorescent phenomenon witnessed by myself while on board the Indian Government Telegraph steamer *Amberwitch*, a few months since, may interest many of the members of the Society. A similar phenomenon has doubtless been witnessed by many others, but I cannot remember having seen any published description of it.

We were steaming out of the Persian Gulf, on Thursday, the 15th May, and at 10 p.m., ship's time, had arrived abreast of Ras Mussendom. The night was fine, but without any moon, and there was just sufficient wind from the S.S.W. to cause a nice ripple on the water; but little phosphorescent effect had been observable in the sea, which at this point is about 50 fathoms in depth. At ten minutes past 10 o'clock the attention of the commander (Captain Bishop) was drawn to a brilliant patch of presumably phosphorescent light, about half a mile distant from the ship, and almost immediately afterwards circular bands of light were observed to be thrown off from the bright centre across the surface of the sea, following each other in rapid succession, like the ripples which occur when a stone is dropped into still water.

The rings of light were obviously concentric, increasing, of course, in their diameter as they receded from the common centre. The rate at which they passed the ship was about 70 per minute, and allowing a distance of forty or fifty yards between each ripple of light the bands must have been travelling from their source at the rate of 3,000 yards per minute, or about 100 miles an hour. This appearance lasted about three minutes, and ceased as suddenly as it commenced, without leaving the slightest trace on the surface of the sea. The bands of the light had an apparent breadth of ten or fifteen feet, the outlines being very clearly defined. I could

Comparison of Battery with Clark's Standard Cells.



$$\text{As R} : 100 :: \text{N} : \begin{matrix}\text{B in terms} \\ \text{of N,}\end{matrix}$$

$$\therefore \frac{100 \times \text{N}}{\text{R}} = \text{B}.$$

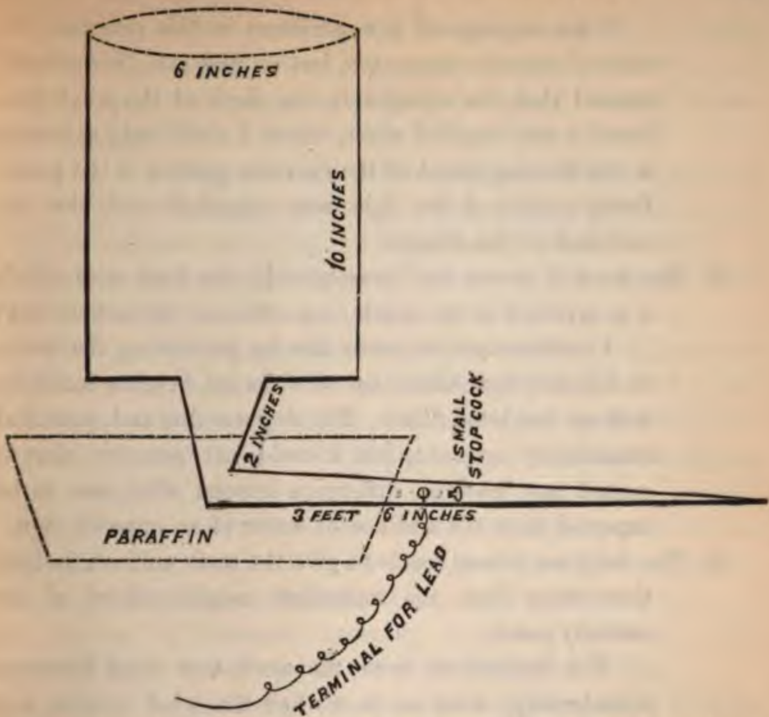
Substituting the head and burning match for the standard cells in the foregoing diagram, we get a new slide reading R_1, the value of which, in terms of the standard cells, is given by the expression

$$\text{R} : \text{R}_1 :: \text{N} : \text{X}$$

$$\therefore \text{X} = \frac{\text{R}_1 \times \text{N}}{\text{R}}$$

I soon found that the matches were open to serious objection. They reduce the point to the potential of the air quickly enough, but care must be taken in rolling them, making them hard and tight, otherwise they burn away before the proper potential has been reached. They do not admit of taking continuous readings, and it is a difficult matter to fix them to the head. Besides this, their use entails an amount of labour on one observer, in running backwards and forwards, sufficient, with the thermometer above 80°, to quell the ardour of the greatest enthusiast.

I therefore procured a water dropper, or rather a rough substitute for it, of the following construction:—



The body and pipe are wholly of tin, being sunk into a block of solid paraffin, which latter is also used as a varnish some distance up the can. The insulation was very good indeed, a delicate Thomson's astatic galvanometer not marking the loss of a division with all available battery. With this apparatus I got what I consider very reliable results. The few remarks I may have to offer as deductions on the observations themselves, and on the meteorology of Pernambuco, consist simply of points which suggested themselves during the course of my reading in conjunction with manipulation.

1. M. Volpicelli states that a fixed metallic point ought invariably to be used in observing the electrical phenomena of the atmosphere, as he believes that a moving point will almost always give indications more or less incorrect.

   Is the water dropper to be termed a movable or immovable point, or does Volpicelli simply refer to the method of Peltier of shooting a point into the air?

I was unprepared to experiment in this direction for want of suitable apparatus, but on the 6th November I noticed that the changes in the force of the wind produced a very marked effect, which I could only attribute to the blowing about of the movable portion of the point. Every motion of the light was coincident with the disturbance of the stream.

2. Has head of water, and consequently the force with which it is expelled at the nozzle, any effect on the indications?

I endeavoured to prove this by permitting the water to fall very low, filling up to different heights carefully with an insulated filler. The day was fine and potential remarkably constant, but I could not perceive that it caused any material difference beyond what was to be expected from the addition of water of an opposite sign.

3. The daily sea breeze seems to give the most uniform indications when from the immediate neighbourhood of the easterly point.

The indications from the north-east wind fluctuate considerably, more so than when the wind is from any other quarter. It is unhealthy and oppressive, though to a new-comer apparently fresh and invigorating. It has a peculiarly deleterious effect on very young children and the young of animals, deaths from convulsions being numerous when it is prevalent.

I almost invariably found that a very low potential at 4 p.m. was followed by rain at night.

With very fine weather the normal potential of the place seems to be about 12 of Clark's standard cells.

4. The readings taken in January and February are based on the statement of Sir W. Thomson that the 11 a.m. reading gives about the mean for the day. Though the means in this series are imperfectly deduced, it will be seen that they approximate closely.

5. My text book for cloud and other coincident observations was Walker's translation of Kaemtz's meteorology, but the variety is so great here, fresh formation and change

of position so rapid, that it is very difficult to classify them properly, therefore many incongruities will be found which will suggest themselves at once to the practised observer.

The presence of a curtain of vapour in the atmosphere at a low elevation, and vapoury streaks at a higher altitude was always accompanied by a high potential.

6. The battery was compared with standard several times during the day.

The climate of Pernambuco does not furnish a very pregnant subject for an observer. The meteorological changes are so few and far between that he seldom or ever gets the chance of observing during any of those startling phenomena which are so common in the temperate zones.

This entire series is necessarily very incomplete, as it is quite impossible in a climate like this for any single observer, residing a considerable distance from the scene of his operations, to devote the amount of attention necessary to secure valuable results nevertheless, I trust they will not prove altogether useless.

[*Note by the Editor of the Journal.*—The tabulated observations that follow are exceedingly complete, and show that Mr. Thomas has thoroughly appreciated the request made to Telegraph Engineers residing abroad, by Sir Wm. Thomson, in his inaugural address as President of the Society in 1874, that they should assist meteorology by using the facilities their position gave them for making systematic observations of atmospheric electricity. The Council have so fully appreciated Mr. Thomas's labours in this direction that they have kept back for a considerable period his tabulated observations, in the hopes of publishing them in full; the considerable expense, however, that would attend this, combined with their desire expressed in their last annual report to economise, induces them to publish now an abstract, prepared by the Publishing Committee, rather than incur further delay.]

Abstract.

The original tables show the hour, the temperature, direction of the wind, state of the weather, state of the sky as regards cloud

potential of the air relatively to that of the earth measured in Clark's standard cells, for every hour from 9 a.m. to 4 p.m. daily. General remarks are also appended regarding the health of the district. The facts that strike one at once in looking over these observations of atmospheric electricity at Pernambuco, are, first, that the potential of the air is almost always positive to the earth; secondly, that for any one day it is extremely constant compared with what one is accustomed to find in England. On the rare occasions when Mr. Thomas found the atmospheric potential negative to that of the earth, there was heavy rain, and the presence of cirro-strati and cirro-cumuli clouds.

The following table shows the *approximate* highest, lowest, and mean atmospheric potential expressed in terms of Clark's cells for each day, the value being always positive unless otherwise stated:—

1876.

Date.	Highest.	Lowest.	Mean.	Date.	Highest.	Lowest.	Mean.
Oct. 11	14	8	11	Nov. 16	17	10	14
„ 12	9	5	6	„ 17	18	10	14
„ 13	8	5	6	„ 18	14	8	11
„ 14	9	5	7	„ 20	11	1	4
„ 15	10	7	9	„ 21	10	3	7
„ 16	9	7	8	„ 22	10	1	7
„ 17	10	5	8	„ 23	15	1	8
„ 18	12	5	9	„ 24	15	2	9
„ 19	11	6	8	„ 25	14	0·5	9
„ 20	13	9	11	„ 27	13	2	9
„ 21	16	7	12	„ 28	13	0	4
„ 23	13	7	10	„ 29	16	5	11
„ 24	12	4	8	„ 30	16	4	9
„ 25	11	3	7	Dec. 1	12	1	7
„ 26	18	12	15	„ 5	10	1	4
„ 27	{ Too high to measure. }	16	...	„ 6	13	2	7
„ 28	15	10	12	„ 7	11	0·5	6
„ 30	13	5	10	„ 8	13	10	11
„ 31	12	8	9	„ 9	13	2	9
Nov. 2	18	12	14	„ 11	15	2	8
„ 3	14	3	8	„ 12	16	1	9
„ 4	9	4	7	„ 13	14	1	9
„ 5	17	10	13	„ 14	14	8	11
„ 6	14	11	12	„ 15	10	2	6
„ 7	10	4	7	„ 16	15	0·5	9
„ 8	9	1	6	„ 18	12	5	9
„ 9	11	2	6	„ 19	9	0*	
„ 11	12	6	9				
„ 12	12	10	11				
„ 13	13	1	9				
„ 14	13	4	9				
„ 15	15	7	12				

* But possibly due to leakage. Observations discontinued from irregular behaviour of the quadrant electrometer indicating a local fault.

During January the observations were resumed after the elec-[t]rometer had been readjusted, but only readings at 11 a.m. were [t]aken. The atmospheric potential was found always positive, daily [o]bservations for thirty days showing results varying from 10 to 18

Clark's cells. One observation was obtained as low as 4·6, but this was accompanied by misty weather seaward.

In order to test the constancy of the readings, Mr. Thomas on certain occasions made five minute readings for an hour, and on other occasions minute readings for an hour. The results obtained were remarkably constant.

A very good example of the connection between the atmospheric potential and the state of the weather, is shown from the following remarks made by the experimenter on October 25th :—

REMARKS ON 25TH OCTOBER.

9.0 a.m.—Between 9 and 10 wind shifted right round, first to E. then N.E.

9.50 a.m.—Very heavy rain squall to S., during which potential fell to 3·40 cells.

10.15 a.m.—Wind gone to S. again, 6·70 cells, sky becoming overcast.

11.10 a.m.—Very heavy rain storm coming up fast. Potential 2·50 cells.

11.50 a.m.—Rain ceased—almost dead calm now. What little breeze there is comes from N.E. Potential 2·40 cells immediately after rain.

12.15 p.m.—*Potential changed to negative* for few minutes, but did not get value.

12.20 p.m.—*Got negative potential for first time.*

Fluctuations very rapid. Immediately after rain—squall potential gradually fell and passed to negative, showing a strong reaction—no wind, very calm and still—little cloud.

Time.		Cells.
12.20	=	4·30 N.
25	=	0·30 N.
30	=	2·80 P.
35	=	1·00 P.
40	=	7·50 P.
45	=	7·10 P.
50	=	8·80 P.
55	=	10·00 P.
1.00	=	10·60 P.

Mr. Thomas does not seem to have been successful in tracing any exact connection between the state of health of the neighbourhood and the electrification of the atmosphere.

In comparing the difference of potential between the atmosphere and the earth, obtained in Pernambuco, with the values observed by Sir Wm. Thomson on the sea beach in the Island of Arran, and on the Links near Aberdeen, one notices the extreme smallness of the former results. But it must be, of course, remembered that the spout of a water dropping collector only projects a comparatively short distance through the window of a house, and even when exposed to the sea is necessarily more or less overshadowed by the house walls, which causes the value of the atmospheric potential obtained to be too low. We must not, therefore, conclude that, because Mr. Thomas found with his apparatus 12 volts as the average potential of the air twenty feet above the ground, while on the other hand Sir William found 30 volts per foot of elevation as a common indication in the open (Papers on "Electricity and Magnetism," Art. xvi., pp. 219 and 229), therefore the ordinary electrification in Scotland is fifty times as strong as in Pernambuco. For in order to determine the absolute value of the difference of potential per foot of elevation, the place must be quite clear of houses and trees, and the observer must also take care, by lying down, to prevent any inductive action of his own body. Mr. Thomas's observations show, however, remarkably well the variations of potential from hour to hour at a given point in the air, the disturbing causes due to houses, &c., remaining always the same.

UNE OBSERVATION SUR LE CONDENSATEUR.

VITORIA, 2 *Aout*, 1879.

On suppose qu'il n'y a pas de courant de terre, et que la resistance du galvanomètre est presque nulle.

M. Varley dit (*Journal of the Society of Telegraph Engineers*, No. 26, vol. viii., page 106) que la 1ere. partie d'un courant de pile ou galvanique, arrivera au condensateur ne faisant pas attention au chemin que lui offre la résistance *R*, et qui forme une dérivation à la terre (*vide* figure 4, page 67 of the Journal) parce que la résistance du condensateur est presque nulle; mais que dès le

moment où le condensateur sera chargé, un courant croissant par moment traversera la résistance *R*, et que le condensateur cessera son travail de chargement avant d'arriver au maximum qui lui est propre.

Voyons si nous pouvons expliquer ce phénomène. Rappelons que l'éminent savant *Secchi* a comparé les ondes galvaniques aux ondes d'une rivière, et les ondes des courants d'induction aux ondes sonores dans cette même rivière, lesquelles effectivement sont extraordinairement plus courtes et plus rapides.

Cette asseveration m'a fait croire qu'une ligne avec condensateur peut être comparée à une rivière avec écluse. La période initiale variable d'un courant galvanique se compare d'ondes électriques toujours croissantes jusqu'à une certaine limite. Ces flux électriques rencontreront la première lame du condensateur et formeront les ondes rapides du courant induit.

Le 1er. flux élémentaire arrivera au condensateur avec toute sa force, sans qu'aucune de ses parties s'écoule par le chemin *R*. Ce 1er. flux après le choc reculera comme reculent les ondes de la rivière quand elles rencontrent l'écluse.

Le 2nd flux rencontrera le 1er. qui recule, et dont la poussée l'obligera à l'écouler en partie par le chemin *R*, et aussi de toutes les autres ondes qui, chaque fois plus embarrassées par les flux qui recoulent, s'échapperont par *R*, ne dépensant dans les parois du condensateur qu'une partie de leur force, venant à resulter que la partie utilisable du courant induit la plus forte et la seule capable de faire le signal dans l'appareil recepteur est celle formée par la 1ere. onde élémentaire du courant galvanique. Les autres s'échapperont pour ainsi dire par le chemin *R*, après leurs mouvements turbulents de chocs et de reculements. *The speed* par conséquent est très favorisé employant la résistance *R*, si la sensibilité du galvanomètre repond à l'intensité du 1er. flux élémentaire du courant de la pile.

C'est de cette manière aussi qu'on pourra comprendre facilement, pourquoi le potentiel d'une pile devient bien amoindri quand le courant arrive au condensateur.

FELIX GARAY,
Foreign Member.

[*Translation.*]

A NOTE ON THE CONDENSER.

VITORIA, *August 2nd*, 1879.

Let us suppose that there is no earth current, and that the resistance of the galvanometer may practically be neglected.

M. Varley says (*Journal of the Society of Telegraphic Engineers*, No. 26, vol. viii., p. 106) that at the commencement of the current arriving at the condenser, the circuit having a resistance R, and which forms a shunt to the earth, produces no effect, because the condenser acts as a circuit having almost no resistance; but from the moment the condenser becomes charged an increasing current flows through the resistance R, and that the charging of the condenser will stop before it has increased to its normal amount.

Let us see if we can explain the phenomenon. To do this we must bear in mind that the distinguished philosopher Secchi has compared electric waves to those of a river, and the waves of current induction to the waves of sound in the same river, which in truth are extraordinarily shorter and quicker.

This assertion leads me to think that a line with a condenser can be compared to a river with a dam. The initial variable period of a galvanic current can be compared with electric waves, increasing continuously to a certain limit. This electric stream meets the first layer of the condenser and sets up the rapid waves of the induced current.

The first elementary stream arrives at the condenser in full force, without any portion flowing by the circuit R. The first stream after the shock bounds back, as do the waves of a river when they meet the dam.

The second stream meets the first which has been reflected, and the return of which causes a portion of the first to escape by the road R, and similarly with all the other waves, which each time, being more affected by the reflected streams, only expend on the walls of the condenser a portion of their power; leading to the result that the strongest and only portion of the induced current

utilised and capable of producing the signal on the receiving instrument is that produced by the first elementary wave of the electric current. The other portions will escape, so to say, by the road R, after the disturbances they experience from shocks and reflections. *The speed*, in consequence, is greatly favoured by employing the resistance R, if the sensibility of the galvanometer accords with the first elementary stream of the battery current.

In this way, then, one can easily understand why the battery potential becomes less weakened when the current reaches the condenser.

FELIX GARAY,
Foreign Member.

NOTE BY THE EDITOR ON MR. MANCE'S LETTER ON "REMARKABLE PHOSPHORESCENCE IN THE PERSIAN GULF."

Professor Perry has suggested that the phenomenon was probably due to some marine animal breaking the surface of the sea and producing the first-noted phosphorescent patch, and that the waves proceeding in all directions from the place of disturbance produced in phosphorescent water, as they passed through it, the instantaneous illumination which appeared, to those on board the *Amberwitch*, to be travelling bands of light. But if the phenomenon were simply one of wave motion in phosphorescent water of about 50 fathoms in depth (which is the depth stated by Mr. Mance in his letter), then Mr. Perry finds, by calculating according to Scott Russell's rule for the velocity of wave-transmission, that the distance separating any two bands, if 70 passed the ship per minute, must have been about 30 yards, and not 40 or 50 yards as described by Mr. Mance. It is, however, of course quite possible that, in consequence of the speed of the patches and the small time they lasted, Mr. Mance may have over-estimated the distance separating the bands.

Each other set of travelling bands crossing the first would, according to Professor Perry's explanation, have been produced simply by a marine animal breaking the sea at another place.

Memoirs.

SIR WILLIAM FOTHERGILL COOKE.

By LATIMER CLARK, M.I.C.E.,
*Past President.**

SIR WILLIAM FOTHERGILL COOKE, an Honorary Member of this Society, who died on the 25th June, was one of those men who, though like Ronalds, little known or regarded by his contemporaries, has contributed an important chapter to the history of the industrial and scientific progress of the 19th century, and has added largely to the fame and reputation of his country. He has at the same time made a name for himself which will grow greater as we recede from it, and which is probably destined to endure through distant ages.

As Stephenson, though not the actual originator of the Railway System, has inseparably associated his name with its introduction, so Cooke, though he contributed but little to the invention of telegraphy, has earned for himself the title to claim the honour of being "entitled to stand alone as the gentleman to whom this

* *Note.*—The writer of this article having been requested to furnish a sketch of the life of Sir William F. Cooke, would, from the pressure of other occupations, have willingly avoided the task, but that he felt that he had sources of information and materials at his disposal such as are not probably possessed by others. The history of Sir W. F. Cooke is the history of the electric telegraph, and having been associated for a great many years with the Electric Telegraph Company, of which Sir William Cooke was a Director, he had an opportunity of acquiring an intimate acquaintance with his character and labours, and had also access to sources of information not open to others. In addition to this Sir William Cooke had some years since presented to him the letters and papers connected with the arbitration between himself and Professor Wheatstone in 1841, and had furnished much information. Colonel Andrewes, of the Royal Horse Artillery, Woolwich, Sir William Cooke's son-in-law, has also entrusted to the writer's custody a series of private letters written by Mr. Cooke during the years 1836, 1837, and 1838, which are of the greatest interest, and which contain the most minute description of the rise and progress of the invention; these documents will be carefully preserved, and

country is indebted for having practically introduced and carried out the Electric Telegraph as a useful undertaking,"—an honour which was awarded to him after an almost judicial investigation of the facts by Sir Marc I. Brunel and Professor Daniell, at the time of the arbitration of the differences between himself and Professor Wheatstone in 1841. This, too, was six years before he crowned his labours by establishing the Electric Telegraph Company, the earliest telegraphic association in the world, and which, under his direction and that of his friends, soon grew to be the largest and most successful.

He survived his contemporaries Ronalds, Wheatstone, and Morse, by only a few years, and with him has passed away the last of that little band of men who have been among the foremost in conferring upon the world the practical benefits of the Electric Telegraph. More fortunate than many benefactors of mankind, they had the satisfaction of living to see their labours completed, and the wildest dreams of their early anticipations realised more than tenfold. Even to those who well remember the pre-telegraphic era, when it took days and weeks to communicate with distant countries instead of minutes, it is difficult to realize the extent of the change which has been produced in human existence, by the joint operation of the railway and the telegraph; to those who come after us, it must be impossible. Others before him had striven

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will be at all times open to the inspection of those who desire to study the subject. The letters to his mother have been carefully preserved; they bear the post-mark and the usual postal inscriptions of the period; they refer chiefly to private affairs and are invariably crossed, and every corner and surface except the exterior is closely covered with writing. Copious extracts have been made from them not without a misgiving that they might prove tedious, but there are so many members of the Society still living who were personally concerned in the introduction of telegraphy, and the subject is in itself one which must always possess so much historical interest, that it has been thought for these reasons best to draw upon them freely, making abbreviations where necessary. We may, in fact, contemplate these early struggles of telegraphic discovery with the same feelings that we regard the birth of the little rivulet which we know is destined soon to gather strength, and to swell into a mighty river. Mr. Cooke was most affectionately attached to his parents, and especially to his mother, and from the minute mechanical and scientific details which are constantly given in his letters to her, it is evident that she must have been a *person of no* ordinary character and intelligence.

earnestly to attract the attention of their countrymen to the importance of the discovery which lay neglected at their feet, but gave up the attempt in despair or were repelled like Ronalds, and driven to retire from the subject in disgust. Cooke was more fortunate, not only in possessing a greater amount of energy and determination, but in falling upon days of great commercial activity, when capital was abundant, and when railways were beginning to spread rapidly, and were enabled to lend a helping hand to the young and rising invention, from which they were soon destined to receive such ample benefits in return. He was further fortunate in making the acquaintance and obtaining the co-operation of many of the most influential and eminent scientific men of his day.

Sir William Cooke was born near Ealing, on the 4th May, 1806. His father, Dr. William Cooke, who died in March, 1857, was a doctor of medicine and professor of anatomy at Durham, and was appointed Reader in Medicine to the newly-organised university there, where he began his lectures in 1833.

His son was educated at a school at Durham, and was subsequently sent to the university at Edinburgh. In 1826 he was appointed to the East Indian Army, in which he held a variety of staff appointments. After nearly six years' service, and while in the Madras Native Infantry, he returned to England on furlough, on account of the state of his health, and he soon afterwards relinquished his appointment. In 1833 and 1834 he betook himself to Paris and attended lectures there, in order to devote himself to those more scientific and practical pursuits for which he felt himself fitted. He studied anatomy and physiology, and practised with great ardour the pursuit of modelling anatomical dissections in coloured wax, an art in which he eventually acquired great skill. In the spring of 1834 he returned to Durham, and prepared a series of models with which his father used to illustrate his lectures at the Durham University; he had even formed the intention of founding an anatomical museum there. In the summer of 1835 he accompanied his parents on a tour to Switzerland. Ascending the Rhine, they visited Heidelberg. Here Professor Tiedeman, the director of the then existing Anatomical Institute, offered to assist him in procuring the means to make his

wax preparations. Accordingly, Mr. Cooke returned in the month of November from Berne to Heidelberg, where he took lodgings in the Stöckstrasse, in the house No. 97, at that time belonging to the brewer Wilhelm Speyrer, but now to the brewer Georg Muller.* It bears the strange inscription, "Bierbraueri zum neuen Essighaus." There had been before vinegar works in that house. As Mr. Cooke was not permitted to make here, in the cleanly-kept apartments, anatomical dissections, he hired a room in the same street, nearly opposite, in the house of the gardener Schwarz, No. 58, now belonging to his grandson, Ferdinand Koch. Here he was during the winter so active, that he was able to dispatch four cases full of models to his father at Durham.

"In the present Anatomical Museum at Heidelberg, I have," says Dr. Hamel, "under Nos. 382, 383, and 628, found three wax models made by Mr. Cooke during the winter mentioned; the last is marked W. F. C. Dunelm."

In the beginning of March, 1836, Mr Cooke heard accidentally from Mr. J. W. Rizzo Hoppner, who afterwards became a member of Mr. Robert Stephenson's engineering staff, that the Professor of Natural Philosophy there had an apparatus with which he could signal from one room to another—this was Baron Schilling's telegraph.† Mr. Hoppner's father was an intimate friend of Lord Byron's, and in one of his letters to his publisher, dated February 20, 1818, he remarks—"Mr. Hoppner has been made the father of a very fine boy; mother and child are doing very well indeed." And the poet wrote four lines upon his birth, which have been metrically translated in ten other languages.‡ The Professor was Geheime Hofrath Muncke. He had, in the upper storey of the former convent of Dominicans where he gave his lectures and where also he lived, suspended wires for telegraphing out of the cabinet into the auditory. "I have examined these localities," continues Dr. Hamel, "and the rooms are now quite empty." From the year 1850 to 1852 the house had served as a military

* See Dr. Hamel's "Historical Account of the introduction of the Galvanic Telegraph." London, August, 1859; a most interesting little work.

† Baron Schilling died at St. Petersburg, 7th August, 1837.

‡ *Murray's* "Poetical Works of Lord Byron," p. 571.

barrack. Mr. Hoppner took Mr. Cooke on the 6th March, 1836, to Professor Muncke's lecture-room.

Mr. Cooke himself thus writes, in 1840, of this period :—* "About the 6th March, 1836, a circumstance occurred which gave an entirely new bent to my thoughts. Having witnessed an electro-telegraphic experiment exhibited about that day by Professor Muncke, of Heidelberg, who had, I believe, taken his idea from Gauss, I was so much struck with the wonderful power of electricity, and so strongly impressed with its applicability to the practical transmission of telegraphic intelligence, that from that very day I entirely abandoned my former pursuits, and devoted myself thenceforth with equal ardour to the practical realisation of the electric telegraph. Professor Muncke's experiment was at that time the only one I had seen or heard of; it showed that electric currents being conveyed by wires to a distance could be then caused to deflect magnetic needles and thereby to give signals. It was a hint at the application of electricity to telegraphic purposes, but provided no means of applying that power to practical uses. His apparatus consisted of two instruments for giving signals by a single needle, placed in different rooms, with a battery belonging to each; the signals given were a cross and a straight line marked on the opposite sides of a disc of card fixed on a straw, at the end of which was a magnetic needle suspended horizontally in galvanometer coils by a silk thread." Either the line or the cross could be exhibited by its motion according to the direction of the current. The apparatus was worked by moving the ends of the wire backwards and forwards between the battery and the coils. "Within three weeks after the day on which I saw the experiment, I had made, partly at Heidelberg and partly at Frankfort, my first electric telegraph of the galvanometer form, which is now at Berne." This telegraph had three needles and six wires in three distinct circuits, with three keys and a rudimentary switch : it gave 26 signals.

Mr. Cooke thus writes to his mother on this subject in a letter

* "The Electric Telegraph. Was it Invented by Professor Wheatstone? By William Fothergill Cooke. London: Smith & Son, 1856. 2 vols. 8vo. Vol. ii., p. '4.

from Heidelberg, dated April 5th, 1836, to Mrs. Cooke at Berne:—

"You must know that for some weeks past I have been deeply engaged in the construction of an instrument which I believe may prove of sufficient importance, should I succeed in bringing it to practical perfection, to merit a visit to London. Determined to satisfy myself on the working of the machinery before I went any further, I prepared to make a model, and being unable to obtain the requisites at Heidelberg, I sought them at Frankfort. Whilst completing the model of my original plan, others on entirely fresh systems suggested themselves, and I have at length succeeded in combining the *utile* of each, but the mechanism requires a more delicate hand than mine to execute, or rather instruments which I do not possess. These I can readily have made for me in London, and by the aid of a lathe I shall be able to adapt the several parts, which I shall have made by different mechanicians for secrecy sake. Should I succeed, it may be the means of putting some hundred pounds in my pocket. As it is a subject on which I was profoundly ignorant till my attention was casually attracted to it the other day, I do not know what others may have done in the same way; this can best be learned in London. You see I am very mysterious at present, and think it very prudent to continue so; nevertheless to you, dearest mother, if it were your wish, my plan and instrument should be explained now, though I think without better drawings than I could make you would scarcely comprehend me. As I do not wish my motives for re-visiting London to be generally known, you had better, in mentioning it to any friends at Berne, state that private business requires my presence, and allow them to ascribe to modelling or what they please the sudden change of my plans."

The original plan he here refers to was the galvanometer telegraph, but the "fresh systems" which have suggested themselves and which are fully described later on, refer to his chronometer escapement or "mechanical" telegraph, which consisted of two musical boxes with lettered dials working synchronously like the Ronalds' telegraph; the mechanism being started and stopped by an electro-magnet and detent opposite the

required letter. This instrument which is frequently referred to hereafter, and which is described and figured in the two volumes before referred to,* occupied his thoughts from this time till May, 1837, but owing to his want of the requisite mechanical skill and assistance, it never at any time performed satisfactorily. In the hands of others, however, the chronometric principle has been most successfully extended and brought into practical use, as in the Hughes instrument.

Mr. Cooke says,† "Before the end of March, 1836, I had invented the alarum, worked by clockwork-mechanism, by the removal of a detent with a magnet in close proximity to an armature of soft iron forming the tail end of a lever detent. When an electric current passed round the voltaic magnet the magnetism which was for the moment excited in it attracted the tail end of the lever, and by so doing drew its detent out of the clock. It was replaced by a reacting spring or balance weight."

He adds, "The first idea of it suggested itself to my mind on the 17th March, 1836, during my journey from Heidelberg to Frankfort, when reading Mrs. Somerville's work on the Physical Sciences."‡

On the 14th of April, he writes to his mother, "On Monday my packages started for Berne by the waggon. They consist of a packing case containing models, and a small box containing instruments, &c," and on the 15th his passport shows that he was himself on his way to London, where he arrived on the 22nd April, and at once commenced his operations.§ On the 26th he writes :—

"I have at length found myself once in London again. I dined with Mr. Fergus, M.P., and there met Mr. Hoppner, who gave me your last letter, which had reached Heidelberg after my departure. Relative to my being in England and the cause, I will explain the whole plan to D. when we are together, but object to so doing on paper, or its being generally known to our friends, as in case of

* Cooke, vol. i., p. 31; vol. ii., 217. † Cooke, vol. ii., 17.

‡ "The Connexion of the Physical Sciences," by Mrs. Somerville, London, 1834. There does not appear to be anything in this work which could have directly suggested the machine devised by Mr. Cooke.

§ Cooke, vol. ii., 18.

failure (always a strong probability), remarks, warnings and advice are the more overwhelming. I have written to Tom begging him to prepare himself with one branch of the subject, and when I have finished my instruments, I will divulge my whole plan to him, and perfect together such papers and statements, &c., as will be necessary ere I proceed further. I have not yet fixed upon any patron (a very important consideration) as the commercial and political world are equally concerned. I have a choice between Government and the mercantile potentates. Whatever course I eventually take will be directed by the best advice," &c.

On the 2nd June he says: "I heard from the Doctor a few days ago. He wrote me a most delightful and affectionate letter. I have explained the instrument and its uses to Robert who will explain all to you."

And on the 6th: "Tom* came up to dinner on Saturday well pleased to shake off for a time the monotony of Cambridge. We commenced work immediately and by mid-day yesterday, he became fully master of the affair. Although it be impossible to give you even an imperfect idea of it in writing, he will give you his idea of its importance and practicability. I am anxious to complete my instrument as quickly as possible. I fear two months will pass away ere I can hope to lay plans and instruments before the public . . . Tom and I are going to the Adelaide Gallery to study various scientific instruments connected more or less with our object in hand . . . It is impossible to describe the comfort and delight of having him to consult and talk to. My confidence has risen sevenfold."

On the 21st July, 1836, he first mentions his sketch or prospectus of his invention, which was intended to be published as a pamphlet, but was not printed. It is headed "Plans for establishing a rapid telegraphic communication for political, commercial, and private purposes, in connection with the extended lines of railroads now in progress between the principal cities of the United Kingdom, through the means of Electro-Magnetism. By W. F. C." It is given at length in vol. ii., at p. 241, and is an interesting document from the period at which it was written.

* His brother, the Rev. Thomas Fothergill Cooke, M.A.

His instrument is to give 60 letters, numerals and signs, and he describes the use of his testing galvanometer or "detecter" much as Ronalds had done in 1823. He speaks of cipher codes, and points out the benefits such a system would confer on the Government, the seaports, the railways, the stock exchange, and the post office. The letter is as follows: "July 21. I think that three months must elapse ere I can know the fate of my projects; one instrument will be shortly so far advanced as to enable me to see whether it answers my expectations. I must then have a second made, and both finished before laying them before the public. The difficulties you allude to of securing the wires were the first I surmounted before thinking further of the instrument, and having succeeded to my content in that respect I then worked out the remainder. My prospectus is ready, but I am about to send it down to Mr. Chevalier, with detailed drawings, for his judgment and correction. . . . I will shortly send you my prospectus, but its length will occupy two or three very closely written sheets, and I have not time for that at present. I will only say that if the wires were broken anywhere between London and Portsmouth I would find the injury out and repair it in less than eight hours. When you see the prospectus and explanations attached you will be convinced of that. A guard or watch is out of the question, but the mode of discovering the injury when done is both rapid, easy, and decisive. Still I beg you seriously not to be too sanguine of my success. I do not know yet that my instrument will answer, and then very probably it may never be used during my life. I fully believe that the day will come when such a means of conveying intelligence will be employed."

Mr. Cooke states, at vol. ii., page 22, that he had at this period given considerable attention to the escapement principle, or step by step movement, which was subsequently brought to such perfection by Wheatstone, and he gives, at plates 2 and 7, a drawing of his instrument.

By August, 1836, Mr. Cooke's account books show that he had already expended £361 14s. 8d. on his experiments.

On September 2nd he says: "My model is progressing. I hope to see it work before I cross for the Continent; it is now so *near completion that I* may speak with confidence of its answering

its destined purpose. It is a very showy-looking affair; the Doctor was much pleased with it."

On the 7th October, writing to his mother at 7, York Buildings, Hastings, he says, "My clockmaker has again disappointed me. I called in Clerkenwell on Monday evening in full expectation of finding that everything had been completed several days; but he told me the balance work had been broken by the running down of the works, so I must wait again." And on the 22nd November, "My instrument was to have been finished this morning, but upon calling I found that a wheel in the escapement movement was wrong and had to be altered. I am to have it on Monday. You would be astonished at the stoicism with which I bear these repeated and endless disappointments. The instrument looks beautiful—I hope it will go as well."

On the 24th he gives Mrs. Cooke a very interesting account of his first visit to Faraday, to whom he explained his plans and his difficulties: "My motive for writing to-day, at a moment when much pressed for time, is to give you a little more cheering news about my instrument than I could do in my last. At that time I had quite despaired of success, and only wished for a justifiable opportunity of giving up, from an impression that I had detected an error in the principle itself. I could not at the moment make up my mind to tell you. Tom will understand and explain this. I had good reason for believing that it was a well-ascertained fact that the galvanic fluid only imparted a magnetic quality to cold iron or an electro-magnet when its course was short, and that the shorter the course the greater the attractive power, hence magnets were made with several short thick wires for experiments requiring *quantity*, and one long thin wire when intensity was required;* to

* See Peter Barlow, F.R.S., "On the Laws of Magnetic Action as Depending on the Length and Dimensions of the Conducting Wire."—*Edinburgh Phil. Journal*, January, 1825. Professor Barlow says:—"In a very early stage of electro-magnetic experiments it had been suggested that an instantaneous telegraph might be established by means of conducting wires and compasses. . . . . I was therefore induced to make the trial, but I found such a sensible diminution with only 200 feet of wire as at once to convince me of the impracticability of the scheme. It led me, however, to an inquiry as to the cause of this diminution, and the laws by which it is governed." Mr. Cooke had perhaps *seen this* paragraph.

set this point at rest I got an introduction (through Dr. Uwins) to Faraday the 'king of electro magneticians.' He received me yesterday and I asked him to give me his opinion upon the instrument, and in the kindest manner he proposed calling this morning for half-an-hour, which he did, but stayed an hour and a quarter, entering with great interest into all the details. He finally gave me as his opinion that *the principle was perfectly correct*, and seemed to think the instrument capable when well finished of answering the intended purpose. He would not give me an opinion as to the distance which the fluid might be passed in sufficient quantity, but observed that if it were only for ten or twenty miles it can be passed on again. He said in reply to my question, 'I am afraid of inducing you by my advice to expend any large sum in experimenting, but it would be well worth working out, and a beautiful thing to carry on in this manner a conversation from distant points, and the instrument appears perfectly adapted to its intended uses.' Now I consider this highly satisfactory. He took his leave in a most friendly manner, but in a way which induces me to think that he does not mean to take any further step in the affair. I asked his advice as to my way of proceeding in bringing it before the public when completed, but he declared his inability to advise me . . . . The difficulty of arranging the escapement I shall be able to overcome without a doubt."

On the 28th November he writes: "I have given Moore my new plan for the escapement. He acknowledges its superiority to our former plans. Faraday could not speak as to the distance the fluid can be conveyed, as he does not know himself; experience alone can prove this. The most minute quantities have been conveyed 4½ miles without any perceptible abatement of their force. The Germans are of opinion that a thousand miles would make no difference. Faraday says that remains to be proved, but does not deny the position. I am still of opinion that the only way to ensure success is to part with some of the advantages to be derived from it to those who are to bring it forward. You are right, dearest mother, not to be too sanguine; the projectors of an original undertaking hardly *ever* yet reaped the benefit of it. The steam

engine, gas, &c., ruined the inventors; the electro-magnetic telegraph shall not ruin me, but will hardly make my fortune."

On the 8th December he writes to his father, Professor Cooke, M.D., Hastings, and for the first time mentions Mr. Walker, who afterwards aided him so effectually by introducing him to Railway directors and others. These letters serve to show the business-like manner in which he conducted the affair. "You will be surprised," he says, "to see my handwriting again so soon, but an idea has crossed my mind, and as you can perhaps advise, I will not lose a post in imparting it to you. I have heard nothing more from Faraday, and in this dilemma I thought of Mr. Walker, whose tastes, connections, situation in the neighbourhood of railroad, and influence and friendship for you render it possible that he may interest himself about my telegraph. . . . If you think Mr. Walker a likely person, you may state the circumstances connected with my plans. My invention has a national and commercial object in view. The instrument and principle coming peculiarly under Faraday's department have been approved by him. I am perfectly willing to explain all my views and obtain, if possible, a written opinion from Dr. Faraday. My wish would be that he should unite with us, furnish the means of proceeding with the patent and instruments, receiving in return shares. You may mention the nature of the instrument, in which case you had better add that the methods of securing the wires, and of detecting injury done to them, have been carefully arranged to Faraday's satisfaction. The instrument I have made is a working one of full dimensions; the expenses attending a patent and experiments, even if tried on an extended scale of many miles, will not, I imagine, exceed £300. My object is to find a person or persons connected with the busy world, who could effectually bring my instrument and plans before the public, and would join me in the expense and profits. . . . I must now acknowledge that dearest mother some months ago mentioned Mr. Walker to me at Paris, but at the time my views were different; to her, therefore, I am delighted to be able to acknowledge myself indebted for so promising a hint, and that seems to give me fairer hope of success."

On the 26th January, 1837, we find a letter from Dr.

Reynolds, Professor at the Royal Institution of Liverpool, to Mr. Joseph N. Walker, in which he speaks of Mr. Cooke's "pamphlet." The letter is given *in extenso* at vol. i., p. 31. On the 28th there is a letter from Mr. J. Walker, of Liverpool, which further refers to the pamphlet:—"I ought sooner to have written to tell you of my having received a parcel from Ravenfield, containing Mr. Cooke's plan for telegraphic communication. . . . . The other reason is that I wished to send Mr. Cooke's papers to two persons whom I thought most competent to give an opinion on its merits. . . . . I sent them to a Mr. Wilson, who is very clever, giving lectures on chemistry at the Mechanics' Institute, and he tells me that he is much pleased with the idea, but he does not know how the instrument can be made to point to such a variety of letters, figures, and symbols. I then sent them to Dr. Reynolds, who is curious in these matters, and well understands this department of science. From him I have not yet received an opinion. I will observe that Mr. Cooke cannot urge his plan (if it can be reduced to a useful practical form) at a better period than this. Already the Committee of the Grand Junction Line are in correspondence on the subject of a telegraphic communication along the line of railway between this and London, and Capt. Watson, the engineer, is supporting a scheme that it is thought may be adopted with effect," &c.

This was enclosed to Mr. Cooke in one from Mr. Thomas Walker, who offers to accompany him to Liverpool, and to introduce him to his brother and other gentlemen likely to forward his views. Mr. Cooke accordingly visited Liverpool, and was there introduced to Mr. Booth, of the Liverpool and Manchester Railway (the inventor of railway grease and of carriage links). He also obtained letters to Mr. Joshua Walker, of London, by whom he was subsequently introduced to Mr. Glyn and Mr. Creed, the Chairman and Secretary of the London and Birmingham Railroad.

He remained some time in Liverpool, endeavouring to obtain the adoption of his instrument on the incline of the Liverpool tunnel, which was then worked by a stationary engine and rope. His instrument, which gave sixty signals, was considered by the

directors too complex for their requirements, and before simpler instruments could be made they were compelled to adopt a pneumatic telegraph, which had been previously under consideration. Nevertheless, soon after his return to London, he had, by the close of April, two simpler ones in working order. His expenditure up to this period was £381 8s. 10d., and he had not yet met Wheatstone.

We now come to a time when he took a step which affected most profoundly both his happiness and his public reputation. He called upon Professor Wheatstone, who had then recently immortalised himself by his exquisitely beautiful method of determining the velocity of the electric current, and, after a few interviews, they agreed to work together in partnership. The glory of Wheatstone's name and reputation at once overshadowed and eclipsed that of Mr. Cooke, who was regarded by the scientific world as a mere business partner (and by some as a mere "practical mechanic"*) whom Mr. Wheatstone had selected to work out his ideas and inventions. Mr. Wheatstone has been accused—with great apparent justice—of giving countenance and currency to these views,† which have become so universal among scientific writers, that until the present generation has passed away, and the subject is examined anew, Mr. Cooke's name cannot hope to receive that credit to which it is justly entitled. No one is competent to speak with authority on this question till he has carefully mastered the two volumes from which we have so frequently quoted, and which contain the arbitration papers and the pamphlets of Mr. Cooke and Professor Wheatstone at full length. These, with two pamphlets issued by the Rev. T. F. Cooke,‡ and the extracts from the private correspondence which I am now giving, afford however, ample materials for forming an opinion.

The letter of Mr. Cooke, of the 27th February, giving an account of his first interview with Professor Wheatstone, is a long

* *Quarterly Review*, June, 1854. † Cooke, vol. i. p. 114.

‡ "Authorship of the Practical Electric Telegraph of Great Britain." By the Rev. Thomas Fothergill Cooke, M.A. Simpkin, Marshall & Co. London, 1868. "Invention of the Electric Telegraph: the Charge against Sir Charles Wheatstone of Tampering with the Press," &c. Simpkin, Marshall & Co., *London, 1869.*

and interesting one. He says: "I tried last week an experiment upon a mile of wire, but the result was not sufficiently satisfactory to admit of my acting upon it. I had to lay out this enormous length of 1760 yards in Burton Lane's small office in such a manner as to prevent any one part touching another; the patience required and fatigue undergone in making this arrangement was far from trivial. From Monday evening till Thursday night I was incessantly employed, and by Friday morning at 10 o'clock all was obliged to be removed. Dissatisfied at the results obtained, I this morning obtained Dr. Roget's opinion, which was favourable, but uncertain. Next Dr. Faraday's, who, though speaking positively as to the general results formerly, hesitated to give an opinion as to the galvanic fluid action on a voltaic magnet at a great distance, when the question was put to him in that shape. I next tried Clark, a practical mechanician, who spoke positively in favour of my views, yet I felt less satisfied than ever, and called upon a Mr. Wheatstone, Professor of Chemistry at the London University, and repeated my queries. Imagine my satisfaction at hearing from him that he had four miles of wire in readiness, and imagine my dismay on hearing afterwards that he had been employed for months in the construction of a telegraph, and had actually invented two or three with the view of bringing them into practical use. We had a long conference, and I am to see his arrangement of wire to-morrow morning, and we are to converse upon the project of uniting our plans and following them out together. From what passed, my plan if practicable, will, I think, have advantage over any of his, but this remains to be proved. Under all circumstances, I should be happy to have a scientific man for my coadjutor, though in that case I must sacrifice a large portion of the advantages. Yet I value his aid much more. You will know him as the person who invented the plan for ascertaining the velocity of lightning by means of a revolving mirror. But for the fear of increasing your anxiety, I should not have written till after our meeting to-morrow, though most likely several days must elapse ere anything definite can be concluded. I cannot say I have enjoyed myself much since coming to London; every moment has been anxiously occupied. I have been engaged

in moving rapidly from place to place, and holding these interviews ever since ten o'clock, and it was four when I commenced writing. I do hope a few days will enable me to decide upon some plan, and either condemn my instrument or set it going. This suspense becomes too wearing. . . . . I will gladly write to dearest B. as soon as anything is settled, but at present all is doubt and uncertainty. In truth, I had given the telegraph up since Thursday evening, and only sought proofs of my being right to do so ere announcing it to you. This day's inquiries partly revives my hopes, but I am far from sanguine. The scientific men know little or nothing absolute on the subject: Wheatstone is the only man near the mark. I cannot explain the point on which so much depends to you on paper. Tom may, when I say that a lengthened course MAY convert *quantity* into *intensity*, in which case no magnetic quality is imparted to the iron magnet, as quantity alone produces that effect. This is a very subtle point on which the 'Doctors' know nothing, and had not thought till I put it into their heads. You will see by this unconnected letter that I am writing in great haste, and with rather a confused head after the fatigues of the day. After dinner I shall be all right again, but at present my stomach wishes to relieve my head by working and allowing its fellow to repose. I shall not regret all this in the end if I succeed at last."

In a letter to his father he remarks: "Faraday and Roget have been very civil to me; the former begs he may know the result of my experiments, as he deems they may lead to more general important results."

At this stage Mr. Cooke had his chronometric dial telegraph, and his galvanometric instrument with six wires and three suspended needles. He had also invented his alarum. We learn from the arbitration papers that Mr. Wheatstone was not more advanced, having only a similar suspended needle telegraph and a permutating keyboard with four keys, by which the circuits could be more conveniently combined and manipulated.* Neither party had made any publication of his inventions. The first to do it was Professor Wheatstone, and under somewhat peculiar circumstances.

* Cooke, vol. ii., p. 25, and Drawing iv.

In the *Magazine of Popular Science*, for March, 1837, there is a long letter from Munich, under the signature of O., dated December 23, 1836, containing, among other scientific information, an allusion to the working of Gauss and Weber's telegraph, and also an allusion to a telegraph on the same plan constructed by Steinheil; the number also contains a list of patents published, including some as late as the 25th of February; it was published some day in the first week in March, about a week after Mr. Cooke's visit to Wheatstone. At the bottom of the last page, just under the notice of Gauss and Steinheil's telegraph, is a short article, included in brackets, containing a reference to some telegraphic experiments performed by Wheatstone about six or seven months previously, evidently inserted after the remainder of the articles had been completed and set in type. The information given could scarcely have come from any one but Professor Wheatstone himself, and by his references to the paragraph he has tacitly admitted its authenticity, and it is hardly possible to doubt that having been permitted to see the proof sheets of Gauss, Weber, and Steinheil's telegraphic experiments, and having, on the 27th of February, been visited by Mr. Cooke in reference to the same subject, he furnished the editor with the notes in question, which were inserted at the last moment in brackets. The question is only of interest because it has been made the ground of a statement that Professor Wheatstone had published an account of his experiments before he became acquainted with Mr. Cooke, which is certainly not the case.*

As we are following a strictly chronological arrangement we will, before proceeding further, allude to a resolution which passed the House of Representatives of the United States at this time, viz., on the 3rd of February, 1837—"Resolved, that the Secretary of the Treasury be requested to report to the House at its next session upon the propriety of establishing a system of telegraphs for the United States." In compliance with this a circular was issued on the 10th March by the Hon. Levi Woodbury, asking information as to the propriety of establishing such a system; the circular alludes to communication "by cannon or

* See Cooke, vol. ii., 153; and Shaffner's "Telegraph Companion," p. 157.

otherwise, or in the night by rockets, fires, &c.," and had no special reference to electric telegraphs.*

On the 15th of April, Sydney E. Morse, a brother of Professor Morse, and according to Dr. Hamel, the editor of a New York paper,† wrote an article in the *New York Observer* describing a plan for an electric telegraph which consisted of 26 wires, and was perfectly useless for practical purposes. In the appendix to Professor Morse's "Modern Telegraphy," published at Paris in 1867, at page 15 Professor Leonard Gale, who was a partner and friend of Professor Morse, and who resided in the same house, gives it in evidence that in March and April, 1837, the announcement of Gonon and Servill's telegraph (which proved to be a visual one) induced Professor Morse to consent to a public announcement of the existence of his invention. If this be correct Professor Morse at this date contemplated a telegraph of 26 wires, but S. E. Morse, at page 8 in the same appendix, makes an affidavit (which was sworn in London in April, 1845) that he wrote the article in question, and though he gave a plan of 26 wires, each a letter, representing this was his own idea and not that of his brother, Professor Morse, and that he was well acquainted with his brother's plan of using only one wire. It is difficult to reconcile these conflicting statements.

Following the order of dates, we return now to Cooke's telegraph. On the 4th March he writes to his mother—"I have not had a moment to spare till to-day for letter writing, even to Treeton, so must be excused for not giving earlier notice of the results of our experiments. Mr. Wheatstone called on Monday evening, and postponed our meeting at King's College till Wednesday. The result was nearly what I had anticipated, the electric fluid losing its magnetizing quality in a lengthened course. An idea, however, suggested itself to Mr. Wheatstone, which I prepared to experiment on last Saturday, but again failed in producing any effect. I gave up my object for the time, and proposed explaining the nature of my discomfited instrument to the Professor. He, in return, imparted his to me. He hand-

* Vail's "American Electro-Magnetic Telegraph Companion," 1845, p. 67.
† "Hamel," p. 62.

somely acknowledged the advantage of mine, had it acted; his are ingenious, but not practicable. His favourite is the same as mine, made at Heidelberg, and now in one of my boxes at Berne, requiring six wires, and a very delicate arrangement. He proposed that we should meet again next Saturday, and make further experiments. For a time I felt relieved at having decided the fate of my own plan, but my mind returned to the subject with more perseverance than ever, and before three o'clock the next morning I had re-arranged my unfortunate machine under a new shape. Tom will understand when I say that I now use a true magnet of considerable power, with the poles about 4 inches apart, and suspended on the plane with its poles by a pivot (like a mariner's compass), a slender armature 4½ inches long, covered with several hundred coils of copper wire (covered with silk). On passing through this coil a stream of galvanism, the armature becomes polarised, and is attracted by the poles of the magnet. The magnet is thus shaped :—The armature is represented by the dots, one end being on this side of the north pole, its fellow on the other side of the south pole. Seen from above thus (gives a sketch):—

"Pivot Z C. The ends of the armature magnetized, Z negatively and C positively. Whenever the galvanic circuit is completed, and worked the ends are respectively attracted by the North and South poles of the magnet with a sufficient force to enable them to overcome the opposition of a feeble spring, the movement will not exceed $\frac{1}{20}$th of an inch. A lever forming part of the detent of my fan is moved by the pin projecting from P, and liberates the clockwork. Tom has seen an arrangement of this description in the Adelaide Gallery, used there merely as a toy. I have determined on making two instruments on this principle, and trying them with Mr. Wheatstone's four miles of wire. Moore has already commenced them. I have made further simplifications of the plan I showed you at Hastings, using no quicksilver triangles or springs, every part restoring itself by balance weights. The cost will not exceed £8, and may be made hereafter for about £2 each. I hope (if I can reduce the cost to this amount) to be able to introduce it into our public offices, banks, &c., and perhaps private houses, as I have a battery in view, four inches cube,

which will continue in action for ten days or more without cleaning (no acid used). I may get it employed on short distances on railroads at once, and experiments may enable us to use it for greater distances hereafter, when the requisite thickness of wire is better understood, and batteries in greater perfection. Thus you see I am deeper in it than ever. I am hastening to a philosophical coterie this evening, where I hope to gain further insight into the construction of the galvanic arrangements."

In this letter we have Mr. Cooke's impressions of the interview at which he and Mr. Wheatstone first compared their plans, but perhaps the most interesting part is that in which he describes his newly conceived form of electro-magnetic relay, the idea of which seems to have been suggested to him not by anything that he saw at King's College, but by an arrangement which he and his brother had seen at the Adelaide Gallery. At any rate, he had now grasped the importance of covering his electro-magnet with "several hundred coils of fine wire" and of having a movement which would "not exceed $\frac{1}{20}$th of an inch;" and the sketch given in his letter also indicates the use of fine wire. It is evident that on this principle (using a permanent horse-shoe magnet, standing vertically, with a light bar pivoted on an axis, covered with fine wire and lying diagonally between its poles and working nearly in contact with them) he had the means of obtaining powerful attractions or repulsions and of working at great distances. Professor Wheatstone, however, who must have seen this arrangement, does not appear to have appreciated its merits, for he afterwards says:* "Mr. Wheatstone succeeded in constructing electro-magnets possessing power sufficient for delicate movements and which acted at very considerable distances;" and again, "His improved electro-magnets enabled him to ring alarums at very considerable distances without the intervention of the secondary circuit which was formerly employed." In his letter to Mr. Cooke of October 20th, 1840, after alluding to the causes of their estrangement, he says; "This led me to resolve to interfere with you as little as possible and to carry on my future researches alone, and to inform you only of the results when obtained. After this resolution had been taken I

* Cooke, vol. i., p. 66.

commenced a series of researches on the laws of electro-magnets, and was fortunate enough to discover the conditions which had not hitherto been the subject of enquiry, by which effects could be obtained at great distances. This rendered electro-magnetic attraction for the first time applicable in an immediate manner to telegraphic purposes." At vol. ii., p. 87, he says: "I saw and told him it could not act, because sufficient attractive power could not be imparted to an electro-magnet in a long circuit, and to convince him of this I invited him to King's College to see the repetition of the experiments on which my conclusion was founded."*

From all this it is evident that Professor Wheatstone at this time did not appreciate the importance of using fine wire, and that he had not studied Professor Henry's paper on electro-magnets in the 20th vol. of *Silliman's Journal* for January, 1831, in which he so clearly shows the advantage of using long fine wires and numerous elements for long circuits.

On the 20th March, there is a letter from Mr. Alfred King, of Liverpool, informing Mr. Cooke that the Liverpool tunnel was 2,250 yards long, and that the apparatus for the air-tube telegraph and the groove in the side of the tunnel were ready: that Mr. Booth did not anticipate there would be any objection to Mr. Cooke making a trial on the terms he proposed.

On the 1st April, Mr. Joseph Walker alludes to the pneumatic telegraph at Liverpool, and says the Directors were disinclined to incur any expense for experiments, but would afford every facility in their power.

On the 8th April, Mr. Cooke writes, "I have been extremely busy with enquiries and experiments on the galvanic battery. I have hit upon one important point, and found a battery upon principles likely to secure my object—of this, more hereafter. I commenced at seven yesterday morning, and never left the room till two a.m. this morning, working without intermission for nineteen hours; this exceeds what I ever did when modelling. My instruments are going on famously, though slowly."

* See also Cooke, vol. ii., p. 93.

11th April.—We learn from Dr. Hamel,* and from the Smithsonian Papers,† that Professor Henry and Professor Bache visited Europe early in April, and that on the 11th April they called upon Professor Wheatstone at King's College, where "he explained to us his plan of an electro-magnetic telegraph, and among other things, exhibited to us his method of bringing into action a second galvanic circuit; this consisted in closing the second circuit by the deflection of a needle. I informed him that I had devised another method of producing effects somewhat similar; this consisted in opening the circuit of a large quantity magnet by attracting a movable wire by a small 'intensity magnet,' or magnet wound with fine wire." We thus see the use of fine wire for magnets again thrust upon Professor Wheatstone, and yet it was long after this that he convinced himself of its importance. The mention of the secondary circuit is important, as long after this date, and after many personal conferences with Professor Henry at Princeton, Morse patented the relay circuit, and claimed to be the first inventor of it.

On the 13th April, Mr. Cooke writes to his mother, "I have left Bury Street, being in need of an extra room to try galvanic experiments, which I am conducting with all the minuteness, accuracy, and ingenuity I can summon to my aid; the result will, I flatter myself, be the most efficient battery for experimental telegraphic purposes yet brought to light. I can keep up perfect steadiness of action for a great length of time, and the moment I have done with it, a touch of the hand removes it from the action of the acid, and by exposing the plates to the air restores them to their original activity. My instruments will be finished early next week."

On the 15th April, the letter of Professor Morse's brother about the telegraph with 26 wires appeared in the *New York Observer* as before described.

On the 25th Mr. Cooke writes:—"Instruments progressing rapidly. I hope my next will inform you of ulterior proceedings with regard to the patent. All goes on most promisingly,

* Hamel, p. 62.

† Proceedings of the Smithsonian Institution, March 16, 1857, p. 32.

particularly my galvanic arrangements, which promise great things." On the same date, there is a very long and interesting letter from Cooke to Wheatstone about their respective claims to the invention of the relay circuit, recalling the circumstances, and showing that they had both apparently thought of it at the same time independently, and by slightly different methods—Cooke, by his electro-magnetic bar, repelled by the poles of a permanent horse-shoe magnet; and Wheatstone, by a needle and pin dipping into a cup of mercury. At the close of this month, Cooke had two of his simpler form of chronometric instruments working together.* On the 3rd of May he writes:—"I do congratulate myself sincerely on being able to give you something like good news respecting my instruments, both of which I have had at home, and in working order since Saturday. By a better and more comprehensible method than that I explained in my letter from or after my return from Liverpool, I obtain 41 signals, and I may as well give a slight sketch of my plan, as it is now arranged, which Tom will further illustrate. My barrel has seven pins, corresponding with as many movable keys, each giving me a distinct signal."

The description of this instrument is a very long one, though it does not explain the interior mechanism. He gives a sketch of a revolving dial plate having fourteen letters and figures on the circumference, in seven divisions. An index moved by an electro-magnet through a short distance indicates which of the two figures is to be read, being actuated by positive and negative currents, but it is not clear how the 41 signs are to be given. It is strange that with so many different characters he does not propose to make it capable of transmitting the whole alphabet, but omits many of the letters, substituting figures and signs in their stead. Upon the whole, the instrument, the result of such long cogitation and experiment, is disappointing, and one is not surprised at Wheatstone, with his exquisite mechanical appreciation, criticising it as severely as he did. The principle was good, and in his hands it might at once have been made a great success; but not being the father of it he appears to have treated it with indifference.

On the 4th May he writes:—"My leaving town to-morrow

* Cooke, vol. i., p. 83.

depends on the result of a meeting I am to have with Professor Wheatstone. I had a conversation through the two telegraphs yesterday."

On the 6th May letters pass between the two partners with respect to the payment of the cost of their new patent, which was applied for at about this date.

On the 11th May he writes:—"At the King's College I was introduced to the Messrs. Enderby, enterprising, determined men, who say there will be no difficulty in raising the capital, &c. As I may hereafter find it convenient to have my plans and improvements established by letters bearing post-mark, I shall describe what I did for Tom's immediate satisfaction, and my own ultimate security. The changes being on the old principle—not the chronometrical arrangement—they are of so promising a nature that the old discarded plan will probably take the lead in some cases. You understood the plan by which I made four wires complete 3 circuits, and, by reversing the same, 3 more. . . . . By pausing double time on any of the contacts a second signal may be represented by each wire, making the number of signals 12. By connecting two lines of wire with one pole a fresh variety of signals may be given. Now my present arrangement is this:— W T (in Fig.) is Wheatstone's wire rope consisting of 4 wires, each covered with tarred flax, and then enclosed in a rope."

He then describes a peculiar commutating key acting on three galvanometer needles suspended on silk threads; it is described as capable of giving 60 signals and rang an alarum. The passage about making a second signal by "pausing double time on any of the contacts," reminds one of the Morse alphabet. On the 23rd May, he says: "I heard from Mr. Enderby yesterday that Mr. Joshua Walker seemed favourable to the undertaking. The Enderbys are very wealthy, they have a large sail-cloth and rope manufactory at Greenwich. I am going down to-morrow to see their works and arrange a method of covering our wires with rope yarn, and include them in a rope for our cross Thames experiment. I sent the wire off yesterday. This rope, 1,500 feet in length, including 6,000 feet of wire, is to be ready by the close of the week. My two new instruments, on the Heidelberg plan,

are in a great state of forwardness. Wednesday will see one finished and the other nearly so . . . I am making a model of Professor Wheatstone's instrument. After seeing Mr. Walker next week, I shall, if encouraged by him, proceed with a rope and wires for the London Terminus of the Birmingham Railroad, where they require a signal communication. Hoppner furnishes me with the measurement, &c. The same rope I send to Liverpool to exhibit there."

On the 27th May his brother sends him also a long prospectus of the proposed Fire and Police telegraph.

During this month, Cooke's needle instrument was shown to the Solicitor General, together with a pasteboard model of Wheatstone's diagram* or hatchment dial instrument, with five vertical needles arranged in a row and indicating by their convergence in pairs the letters above or below. This plan of suspending the needles vertically on axles, like dipping needles,† was due to Wheatstone and was a very great advance on anything that had been done previously, though it soon simplified itself into the double needle telegraph, and eventually into the single.

On the 10th June, he says: "Enderby Brothers write that they have finished the rope. Yesterday I went to King's College to meet Professor Wheatstone and try my instruments: I had hoped to have our experiments made public to-day, but dare not till the patent is out, as one day's impatience may ruin all." He concludes by a postscript in large capital letters. "P.S.—Patent signed by His Majesty and receiving the great Seal this day!!! June 10th, 1837! all now is safe!"

The patent bears the date of June 12th, 1837.

The patent being completed they were now able to exhibit their apparatus in public, and before the end of the month a bright gleam of sunshine falls upon the scene. Mr. Cooke receives from Mr. Creed, the Secretary of the London and Birmingham Railway, then in course of construction, a very friendly letter of introduction to Mr. Robert Stephenson, adding, "The Chairman would wish,

* Cooke, vol. ii., p. 169.

† The Abbé Moigno and many others erroneously state that Schilling employed vertical needles. Hamel, p. 56.

when Mr. Cooke has completed his apparatus for the experiment, that Mr. Prevost should be present. It is understood that Mr. Cooke will communicate freely with you on this interesting subject."

On the 2nd July we find an account of the results of this introduction, from which we may gather some idea of the energy and enthusiasm which Mr. Cooke threw into all his labours:—

"I could not find time till now to write, every moment being engaged from six in the morning till ten at night. On Friday I saw Mr. Joshua Walker, and imparted my plan for the fire telegraph. He spoke handsomely of it, but recommended my proving the practicability of the general principles before I attempted to introduce a project involving the disturbance of the pavement. I then expressed my wish to try experiments on the railroads. "There," he said, "I can at once assist you;" and within half an hour introduced me to the Chairman and Secretary of the London and Birmingham Railroad. They both entered warmly into my views, and appointed the following day for a further consideration of the subject. To shorten details, by following up every opportunity that offered itself, and urging forward my suit unceasingly, I got through all the forms, had three interviews with Mr. Stephenson, the famed engineer, and got an order for 8 cwt. of copper wire by Friday last; obtained leave to occupy a vast building on the railroad, 65 ft. by 100 ft. wide, and had as many men and all the materials I could require placed at my disposal. The order was: 'Let Mr. Cooke have everything he may require.' By strenuous exertions I succeeded in collecting the above vast quantity of wire, cleared the huge workshop of men and lumber, by the constant labour of from 30 to 40 men, and had nearly half a mile of wire arranged by Friday night. Proceeding slowly on Saturday morning—having to teach all the men employed, viz., eight carpenters, two wire workers, and eight boys, their distinct duties—we got forward more rapidly towards evening, and at five o'clock, when the men left off work, I had about four miles of wire well arranged, and hope to get all nearly done by to-morrow night. You may imagine the task when I tell you that 2,888 nails have en put up for the suspension of the wires. The labour can only

be conceived by witnessing our proceedings. I am anxious to show as much activity and accuracy in my arrangements as possible, or I might proceed more leisurely. The Secretary, however, enquired from the Chairman whether I could superintend the laying down of such a communication along the line to Birmingham, should the Company hereafter determine upon it. . . . . Should my movements progress as I could wish, I hope to show my experiments to Mr. Stephenson on Thursday or Friday, when he will be in town, he having been appointed by the directors to report thereon. My application in the first place was to try an experiment at my own expense, but finding the parties I addressed listened to me, I finally proposed their bearing me through free of expense, which they unhesitatingly have done in the most liberal manner. You must not expect to hear from me before next Saturday, and then only the result of my experiments, as I may not hear the nature of Mr. Stephenson's report for a month."

On the 3rd July Mr. R. Creed writes from the offices of the London and Birmingham Railway Company, which were at that time at 33, Cornhill:—"Dr. Pliny Earle and Mr. Stacey are desirous of being present at the experiment which you propose trying to-morrow morning."

The letter to his mother, dated Tuesday, July 4th, gives an account of this experiment:—"I have completed my line of wires, extending for 13 miles, and shall have about 2½ miles more extending to Camden Town and back, laid down as soon as I try my final experiments. I was hard at work all day yesterday, and towards evening I received a message from several of the London and Liverpool Directors, expressing their wish to see any experiments that could be tried along the line, and they would stay in town another day purposely. I promised to do all I could, and worked till ten at night, and commenced again at four this morning. All my wires were brought to a table at one end of the room, and neatly arranged over night, but I would try no experiment till the morning, dreading lest some of my contacts should prove imperfect. Burton Lane was with me by six this morning, when I applied my battery and tried a length of two miles: first—all *right*; then two more with the last—all right;

then 8, 10, 12, and 13, with the same result. All were tried in about one minute, so that the adjusting of my instruments, and sending of messages through a total of 55 miles, required scarcely as many seconds. I only arranged the simplest of my instruments, and had all ready by twenty minutes past nine.

"I then went home and had a good wash, took one mouthful of breakfast, and got back by ten o'clock, the hour appointed. About twenty of the Directors were soon assembled (Mr. Wheatstone could not be present), so I commenced my explanations, and got through them with all the ease and coolness imaginable. I should not have been less nervous had I been explaining them only to you. I said I had hastened my preparations not to disappoint those Directors who were leaving town, but did not offer them as an example of what my telegraph could do, but to show that the current of fluid would pass through miles of wire instantaneously, &c. I commenced by putting my Heidelberg instrument in motion, which excited great interest. I then rang a bell, &c., and finally displayed the gradual decrease of galvanic energy in lengthened currents by transmitting the current first through two miles, &c., and so on to the 13th. All expressed themselves satisfied with the principle, and seemed to take the deepest interest in the experiments. One final experiment will be made next week. Mr. Stephenson was present, and played with the instrument more than anyone else. We are to exhibit before him and a Mr. Prevost finally. We mean to try whether a machine called an 'Electro-magnetic' cannot be made to supersede the galvanic battery. I have long been anxious to ascertain this point, it being part of my original plan."

The instruments used on this occasion were his suspended needle telegraph and his "mechanical," or chronometric telegraph.

About this time we find a letter from Professor Wheatstone to Mr. Cooke, without date. "As you state that some of the Railway Directors are leaving town on Wednesday, I will prepare the apparatus for two o'clock on Tuesday, though it will interfere with the preparations for my lecture. I received a letter from Mr. Stephenson that he and Bagster would come to the College to · my experiments, but they did not come.

"With respect to your application for some cash I am sorry I cannot at present comply, &c.

"I have spent within the last week or two a good bit for my signal boards, instruments for completing the secondary circuits, alarum, new battery, &c.

"I am anxious to get my last telegraphic apparatus complete, because I am perfectly convinced it will be the only efficient and available one."

On the 9th July Cooke writes to his father at Hastings:—

"Many thanks for your letter, I had seen the same advertisement, by a Mr. Alexander, in the *Times;* there is nothing new in the details and they make no mention of alarums, the very pith of our patent. The result of our experiments far exceeds my anticipations. My instruments for bringing a secondary battery into action at the distance of 14 miles act under the influence of six plates of my battery to admiration. Professor Wheatstone had calculated upon seven plates to a mile and I upon two. I do not think that more than one pair per two miles will be required, and those very diminutive. I turned a needle rapidly 90° with a tip of zinc amalgamated wire, and another tip of copper wire, and a little dilute acid on my finger through a circuit of 14 miles; this wonder I have not yet shown to any one.

"Monday night. Mr. Stephenson and Mr. Creed have been here to day* and took the deepest interest in the experiments; they wish to see the effect in greater distances still, and I have orders to fit more wire and extend along the road. I need not say that each little delay adds a gray hair to my temples and a wrinkle to my brow."

On the 10th, Mr. Creed writes: "I have appointed Mr. Prevost and Mr. Stephenson to meet us at Euston Station at half-past eleven to witness the trial of your experiments," which are evidently those alluded to in the postscript to his letter to Dr. Cooke.

July 19th. We have already remarked that an allusion was made in the March number of the *Popular Science Magazine* to a telegraph by Professor Steinheil of Munich, on Gauss and

* *This must* have been Monday, July 10th.

Weber's principle. It is evident that he continued to pursue the subject, for we learn from the *Comptes Rendus* of September, 1838, in a communication from Arago to the French Academy of Sciences, and from Sturgeon's *Annals of Electricity* for March and April, 1839, that on the 19th July, 1837, Steinheil had made a telegraph on a different construction, and had seven miles of it in operation.* No publication of this experiment appears, however, to have been made till 1838, when it appeared in the *Comptes Rendus*.

On the 24th July, we find a letter from Mr. Prevost, saying, that in crossing the tunnel on the Camden incline to avoid a passing train, he had run against the wires, and recommended their removal to one side. This was the rope with five wires manufactured by Enderby Brothers.

We now arrive at the time of the first public exhibition of Cooke and Wheatstone's telegraph, which is thus described in a letter to Mrs. Cooke, dated July 25th†:—"Yesterday Mr. Stephenson witnessed our experiments through 19 miles of wire, extended from Euston Square to Camden Town, and declared himself satisfied with the result, but begged me to lay down my wires permanently between those two points on my best plan, with a view to extending the communication hereafter, if the directors approved. He also wishes to have all our instruments on the most approved construction, and I have consequently put several new ones in hand. He declared himself a convert to our system, and seemed quite delighted at the correspondence we carried on at so great a distance, requesting me to send the word

* Cooke, vol. i., p. 11. Highton's "Electric Telegraph," p. 57. Vail, p. 179 Hamel, p. 57.

† This would appear to make the date the 24th, but in 1876 Sir W. F. Cooke gave the writer the following note:—"July 25. This experiment was performed in the presence of Wheatstone, Stephenson, and Sir Charles Fox. Brunel and Sir Benjamin Hawes had been there in the evening." There might have been a second and more public trial on the 25th. He also spoke of "a rope of five wires, each wire of copper, and insulated by a covering of rope, and all hung up in a bundle." About this rope, see Cooke, vol. ii. p. 131-2. In the author's copy Mr. Cooke has written:—"Sub-Thames experiment before Prince Albert. The gun was fired by a signal from King's College to Mr. Walker's *Shot Tower*. W. F. C., Jan. 5, 1875."

'Bravo!' along the line more than once. It ended by his desiring me to send an invitation to Mr. Wheatstone to join us, which he politely replied to by saying he would do himself the honour, &c. Mr. Stephenson seems to have taken our telegraphs entirely under his patronage, and a more influential one we could not desire. . . . I have just given orders for 5,000 feet of wood to be sawn in a particular manner, with grooves for the wires, which I am going to have boiled in coal tar previously to laying down.* Our wire is all ready. A variety of notices keep appearing in the newspapers respecting electric telegraphs, but there is nothing in them. Some rumours have got abroad respecting ours, which have given rise to most of them. The one tried at Munich will not answer on a lengthened line, we having tried experiments, and proved the insufficiency of the plan."

The instrument used on this occasion was Wheatstone's diamond-shaped "hatchment" instrument, with five vertical needles,† and we have an account of what was doubtless the same experiment from information supplied by Professor Wheatstone himself to a writer in the *Quarterly Review* for June, 1854 (Dr. Andrew Wynter). This article, after quoting the paragraph in the *Magazine of Popular Science* for March, 1837, says:—"Following up his experiment, Professor Wheatstone worked out the arrangement of his telegraph, and having associated himself in 1837 with Mr. Cooke, a practical mechanic who had previously devoted much time to the same subject, a patent was taken out in the June of that year. . . .

"Late in the evening of the 25th of that month, in a dingy room near the booking-office at Euston Square, by the light of a flaring dip candle, which only illuminated the surrounding darkness, sat the inventor, with a beating pulse and a heart full of hope. In an equally small room at the Camden Town Station, where the wires terminated, sat Mr. Cooke, his co-patentee, and,

* Specimens of this telegraph are occasionally dug up on the Camden Incline, and are known among telegraphists as the "fossil telegraph."

† Cooke, vol. ii., p. 49. The Electric Telegraph Co. *versus* Nott and others. Chancery proceedings, 1846, p. 125. The evidence given in this case is very interesting, on account of the eminence of the persons who made affidavits.

among others, two witnesses well known to fame, Mr. Charles Fox and Mr. Stephenson. These gentlemen listened to the first word spelt by that trembling tongue of steel which will only cease to discourse with the extinction of man himself. Mr. Cooke, in his turn, touched the keys, and returned the answer. 'Never did I feel such a tumultuous sensation before,' said the Professor, 'as when all alone in the still room I heard the needles click, and as I spelt the words I felt all the magnitude of the invention, now proved to be practical beyond cavil or dispute.'"

This article justly gave much offence to Mr. Cooke and his friends, and led to the publication of two pamphlets by his brother, the Rev. Thomas Fothergill Cooke, M.A., the one entitled "Authorship of the Practical Electric Telegraph of Great Britain, Bath and London, 1868," containing 131 pages, and the other, "Invention of the Electric Telegraph. The charge against Sir Charles Wheatstone of tampering with the Press." London, 1869.

In the latter he gives a copy of a letter from the Editor of the *Quarterly Review*, Mr. W. Elwin, who says, "I did not write the article on the telegraph, but I wrote that portion relative to the merits of the respective discoveries of which Mr. Cooke complains—the author of the Essay was prompted exclusively by Mr. Wheatstone.

On the 28th July Mr. Stephenson writes, asking that his experiments may be repeated before two ladies "who have had their curiosity much excited by the accounts which I have occasionally given them of the results of your experiments," this letter by its friendly tone appears to have given Mr. Cooke much pleasure, for he says to his mother, "I received such a friendly letter from Mr. Stephenson last night, asking me to show a few experiments to two ladies staying with them who were shortly going away. I must copy a paragraph. . . . Now, is this not a nice style of letter to receive from such a man? I was much inconvenienced, but laboured late last night, and got all in readiness. Everything acted better than before, and all were delighted. Mr. Stephenson more heartily friendly than ever."

On the 14th August we have the first indication of that jealousy as to the credit to which they were mutually entitled, which after-

wards became chronic, and culminated in the arbitration and award of 1841, and the various pamphlets and publications before alluded to.

On the 14th August Mr. Cooke's solicitor, Mr. Robert Wilson, writes to him, in evident allusion to some paragraph which had appeared in the papers, " So much time had already elapsed it was no longer advisable to insert a correction of the erroneous paragraph. I think that the best way now would be to take the opportunity of correction which would be afforded by the next insertion of a report of experiments." *

We come now to the date on which Professor Morse's telegraph is first made public. We have already seen that on the 10th March, the United States Government had issued a circular on the subject of telegraphs, and that on the 15th April, a letter by S. E. Morse, a brother of Professor Morse, appeared in a New York paper, describing a Morse's system of 26 wires, about which, it was stated on oath, by Professor Leonard Gale (Morse's partner, and resident in the same house with him), that it had been inserted with the express consent of Professor Morse as an announcement of the existence of his invention.† By this time, too, Cooke and Wheatstone's experiments had been widely published. Other telegraphs had been mentioned; thus we have Gauss, Weber's, and Steinheil's in March; Alexander's telegraph had been described at great length in the *Scotsman* newspaper of 1st July, and been copied into other papers and into the *Mechanics' Magazine* of 12th August, as alluded to in Cooke's letter to his mother of July 9th. Mr. Cooke also says, on the 25th July, " A variety of notices keep appearing in the papers respecting electric telegraphy, but there is nothing in them; the one tried at Munich will not work on a lengthened circuit;" and Wheatstone, on the 6th September, writes: " I read an article in the *Chronicle* copied from the *Courier*."

* Mr. Cooke writes of this period: " The invention at once became a subject of public interest; and I found that Mr. Wheatstone was talking about it everywhere in the first person singular. I remonstrated with him. I cautioned him as a friend that he was getting himself into a false position."—" Cooke," vol. i. p. 8.

* *Morse's "Modern Telegraphy,"* Paris, 1867, Appendix, p. 15.

At the end of August, 1837, the *Wurzburger Zeitung*, with an account of Steinheil's doings had reached New York and been translated in a paper there on the 1st September.* Professor Henry, also, who had called on Wheatstone in England and seen his telegraphs, had returned to America and held frequent consultations with Professor Morse at Princeton.†

With all this excitement about telegraphs it is not surprising that Professor Morse should consider it time to make his inventions public. This he did on the 4th September in a letter to the New York *Journal of Commerce*, saying (in allusion perhaps to the letter of April 15th), "You recently announced that I was preparing a short telegraphic circuit to show to my friends the operation of the telegraph; this circuit I have completed of the length of 1,700 feet, and on Saturday, the 2nd, in the presence of Professor Gale and Dr. Daubeny of the Oxford University, and several other gentlemen, I tried a preliminary experiment with the register. It recorded the intelligence sufficiently perfect to establish the practicability of the plan and the superior simplicity of my mode over any of those proposed by the Professors in Europe. No account has reached us that any of the foreign proposed electric telegraphs have as yet succeeded in transmitting intelligible communications, but it is merely asserted of the most advanced experiment (the one in London), that by means of five wires, intelligence may be conveyed."‡

On the 27th September, he addressed a letter in reply to the circular of the Secretary of the Treasury, stating that he had invented his telegraph five years previously, and had contracted with Mr. Vail to have an apparatus working by the 1st January, 1838. He adds: "The cost of a single copper wire, $\frac{1}{16}$ inch in diameter, for 400 miles, was recently estimated in Scotland to be about £1,000 sterling." (This statement is derived from *The Scotsman* of 1st July, 1837). He proposes to use a single wire, either in pipes or suspended on poles, and adds, that in conjunction with Professor Leonard Gale, he is about to prepare a circuit of 20 miles. He

* Dr. Hamel gives another account of this experiment. Hamel, p. 65.

† Hamel, p. 64.

‡ *Proceedings* of the Smithsonian Institution, March 16th, 1857, p. 32. *Hamel, p. 62.*

again writes on the 28th November that he had experimented with complete success, at a distance of five miles with a battery of 57 plates, and had more recently worked ten miles with similar success.

From these letters it is sufficiently evident that the American telegraph grew out of the efforts of Messrs. Cooke and Wheatstone and other European telegraphers, and that there is no ground for that claim of priority which it has sometimes been endeavoured to set up.

On the 6th September Mr. Wheatstone writes to Mr. Cooke from 20, Conduit Street: "Mr. Bagster is extremely anxious about the Train Telegraph, for communicating between the guard and the engineer, two wires will suffice for the communication, the best means of connection between the carriages will require some consideration, and perhaps when you return you will turn your thoughts to the subject, &c. I read a short paragraph this morning in the *Chronicle*, copied from the *Courier*, which has a little annoyed me. . . . It is full of misstatements, owing no doubt to the writer having received his intelligence at second or third hand. The notice states that experiments have been made on the railway, under the direction of Mr. Stephenson and myself, for 25 miles, and then describes in rather an unintelligible way the original apparatus with four wires, insinuating that there is no originality in the invention, but giving me credit for the adaptation."

On the 22nd September we find a letter from Mr. I. K. Brunel, dated "Great Western Railway, 18, Duke Street.—Mr. Brunel presents his compliments to Mr. Cooke, and, if convenient to Mr. Cooke, would be desirous of seeing him on Sunday afternoon at about two o'clock, or on Monday morning at half-past seven," and a letter from his solicitor, Mr. Wilson, of the same date, introducing him to Sir Benjamin Hawes, M.P., who promised at once to bring the invention under the consideration of the Government, and a few days later, experiments were performed before some members of the Government. On the same day Mr. Wilson writes that Mr. George Peabody had called upon him, and was desirous of taking out an American patent, and introducing the invention into the

United States, and on the 28th there is a note in Mr. Peabody's writing,* making an appointment to see the experiments at Euston Station.

About the 28th or 29th September, Mr. Cooke sends in proposals to Mr. Glyn, the Chairman of the London and Birmingham Railroad Company, for the establishment of an electric telegraph from London to Birmingham, Manchester, Liverpool, and Holyhead; the telegraph to be carried into the several exchanges, and to be open to the public generally at such uniform charges as may be agreed upon between Mr. Cooke and the Company; an Act of Parliament to be obtained, and Mr. Cooke to have a royalty of £16 per mile, and a share of the profits. This proposition was submitted by the request of the Company. This is followed on the 2nd October by a letter offering his services for the construction of a telegraph for the Railway Company. He adds:—"I feel confident that Professor Wheatstone will consent to any terms I recommend, and on his return I will talk over the patent licence with him."

The 6th October is the date of the *caveat* of Professor Morse's first patent.

On the 17th Mr. Cooke's solicitor enquires whether the difficulties with Mr. Wheatstone as to priority of names† will throw obstacles in the way of concluding the agreement with Mr. Peabody for the American patent, and he naively remarks:—"The English specification is published in the 'Repertory of Arts' and other works which are sent over immediately to America, and which the Americans will immediately make use of to teach them the invention, only without the ceremony of paying you for it, unless you or Professor Wheatstone choose to 'trans-Atlanticise' for a time." There is a letter from Professor Wheatstone on the subject on the same day, and on the 19th a receipt from Mr. Peabody for a parcel of papers, &c. It does not appear why no patent was ever obtained in America, but Dr. Hamel implies that the chief at the Patent Office was a friend of Professor Morse, and states that the first sentence sent along the Washington

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* See "Cooke," vol. ii., pp. 60, 61.

† Cooke, vol. ii., p. 61.

and Baltimore line in 1844 was dictated by his daughter.* If any undue influence were exerted in this matter, it has to be remembered that an equal injustice was done to Professor Morse, who came to Europe in 1838 to get his apparatus patented, and was refused an English patent on the ground of want of novelty.

(To be continued.)

THE LATE MR. R. S. BROUGH.

(Member of the Society of Telegraph Engineers.)

MR. RICHARD SECKER BROUGH, youngest son of Thompson Brough, Esq., M.D., was born at Kiltegan in the county of Wicklow, on the 17th October, 1846. He received his early education at a private school in Jersey, kept by a Mr. Thompson, and later on was removed to the Victoria College. In a society like that of Jersey, where in those days so many retired military officers used to settle, and where the troops garrisoning the island are held in such high esteem, most boys naturally inclined to the army as the profession of their choice, and he, more especially as his two elder brothers had already joined the military service, inclined strongly towards this direction also. However, it was decided best that he should study for the Indian Civil Service, and with this object in view he was sent to a Mr. Thompson at Avranches, in France, where he could have more individual attention bestowed upon him than he could possibly receive in a large school such as the Victoria College, even ably as it was ruled in those days by Dr. Henderson, LL.D., whose prim yet kind manner, and impartial treatment of all, rendered him a great favourite with the boys. The young Brough left this place rather reluctantly for his new abode in France, where he quickly however acquired a very perfect knowledge of the French language. At Avranches he studied especially Italian, German, natural and physical sciences, and showed a permanently increasing interest in the study of mathematics. It was then that his great ability for physical and mathematical

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* Hamel, p. 70.

research began to be perceived. He went back to London to continue his study at the Civil Service College (Hodgson's). However, having begun rather late to study for the Civil Service, he had had, notwithstanding his great natural ability, scarcely sufficient time to accumulate that large amount of positive knowledge which a man requires to pass successfully through the ordeal of an Indian Civil Service examination; he failed, and being over age, another profession had to be selected for him.

It was about this time that Sir Stafford Northcote had introduced the present system of examination for the Indian Telegraph Service. Mr. Brough obtained a nomination for that service, and, after another year's study at Portsmouth and London, passed the examination first in his lot. He was appointed by the Secretary of State in Council for India, a fourth grade Assistant Superintendent with effect from the 30th October, 1869. On his arrival in Calcutta, January 8th, 1870, he was posted to the telegraph stores and workshops for instruction. His ability was so marked that he could be appointed Assistant Superintendent of Stores as soon as May the 5th, 1870. Shortly afterwards, in November, 1870, he was transferred to Madras to take charge of that important telegraph office. In March, 1871, he was recalled to be appointed an Assistant to the Superintendent Electrician, which important position he has kept up to the time of his death. In the electrician's office, where physical experiments of varied nature are daily executed, he had all the opportunity to develop his special talents, which he did with so marked a success. During Mr. Schwendler's absence from India he was always latterly deputed by the Director-General to officiate for that officer, not on account of his standing in the graded list, but merely on account of the very great ability which he had shown in all the technical branches connected with that great telegraph administration. His latest practical work, in connection with the Indian telegraph, was his trip to Paumben, undertaken with the special purpose of testing and repairing the faulty Paumben cables in connection with the Ceylon cable, and which he carried out most successfully.

Mr. Brough published the following papers in the Proceedings of the Asiatic Society of Bengal :—

In 1877, "A theoretical deduction of the best resistance of a Telegraph Receiving Instrument."

"Note on Professor Graham Bell's Telephone."

"On a case of lightning; with an evaluation of the potential and quantity of the discharge in absolute measure."

"On the diameter of the wire to be employed in winding an electro-magnet in order to produce the maximum magnetic effect."

In 1878, "Magnetic elements for northern India."

"On the proper relative sectional area for copper and iron lightning rods."

He published a small book containing useful formula for practical telegraphy, which afterwards, being extended, were incorporated in Molesworth's Engineering pocket book under the joint name of Higgs and Brough, and he further brought out, by order of the Director-General of Telegraphs in India, the second revised and amplified edition of Schwendler's Testing Instructions.

Having all the special qualities to become probably in time one of the most eminent physicists and Telegraph Engineers, his sudden death from cholera at Calcutta on the 3rd April, 1879, is much to be deplored. The night before his death he attended, in apparently full health, a meeting of the Asiatic Society, where he assisted in shewing experiments in connection with a paper on "A new standard of light," read by Mr. Schwendler.

Mr. Brough was one of the most active members of the Asiatic Society of Bengal, and since 1871 has been a member of the Physical Science Committee of that Society; he was also an Associate Member of the Institution of Civil Engineers, one of the original Members who formed the Physical Society of London, and a Member of the Society of Telegraph Engineers.

Great as his natural ability and accumulated knowledge have been, they were both surpassed by his kind and generous disposition, especially shewn towards those who had the fortune to serve under him.

// JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. VIII. 1879. No. 29.

The Seventy-ninth Ordinary General Meeting of the Society was held on Wednesday Evening, November 12th, at the Institution of Civil Engineers, 25, Great George Street, Westminster—Lieut.-Colonel BATEMAN-CHAMPAIN (President) in the Chair.

After the transaction of the preliminary business,

The PRESIDENT rose and said: Before proceeding to the regular business, I will ask permission to offer one or two brief remarks. Since we met last May, delegates from the various states of Europe assembled, as you know, in London, to arrange the new Convention for International Telegraphy throughout the world. We, the Society of Telegraph Engineers, had the pleasure of receiving them at the South Kensington Museum, and I have to state, on the best authority—that of the delegates themselves—that they were immensely pleased and gratified at the—to them—very novel entertainment presented to them. On that occasion the expenses of the conversazione were defrayed by subscriptions from the members generally, and owing to the excellent arrangements of the sub-committee—although the entertainment was given fully on the scale that was intended—yet a considerable sum of money (about £200) was left over after all expenses had been defrayed. A circular was then addressed to every subscriber of a larger sum than one guinea, and he was asked whether he would object to the balance being devoted to the interests of the Society, and especially towards defraying certain expenses con-

nected with the Ronalds' Library, or whether he would prefer to have his share of his subscription returned to him. I am happy to say that in every instance the subscribers expressed themselves delighted to leave the money in the hands of the Society, and this sum of £200 has been of very great use to the Council in arranging for the outlay which we were bound to incur in connection with the library. One of the conditions on which we acquired that valuable bequest was, that the volumes and pamphlets should be properly bound, and the estimate for this work came to nearly £200, so that we have been able to fulfil one important condition without touching the funds of the Society. In other respects, the finances of the Society are in a very satisfactory condition. A loan was made by certain of our members a short time ago to clear off some printing debts, and enable us to start the printing and other work of that description in a more economical way. Of that loan, although we only agreed to pay it off in seven years—or one-seventh every year—we have been able, owing to the good management of the Finance Committee, to clear off one-third instead of one-seventh this year. The number of new members is also satisfactory. You have heard this evening the list of new names proposed, and I think we may fairly congratulate ourselves on the condition and prospects of the Society. Before I sit down I desire to give expression to our sorrow at the loss of a very distinguished honorary member who died last June. I refer to Sir William Fothergill Cooke. We hope to have, in a forthcoming number of the Journal, a memoir of this gentleman by a very distinguished member of the Society. I will only say that Sir W. Fothergill Cooke's character and career are well known to most present. He commenced his career in telegraphy so far back as the year 1836. He was one of those who shortly afterwards founded the Electric Telegraph Company, and was, I believe, for many years one of its directors. As I have said, he was an honorary member of our Society, and many present can testify to the uprightness and worth of the gifted man whose loss we deplore.

The SECRETARY then read a paper on "The Telegraph Line from Bushire to Teheran," by Mr. J. H. Preece, &c.

TELEGRAPHS IN PERSIA.

By J. R. Preece, *Hon. Secretary for Persia.*

In the double number, XIII.-IV., of the Society's Journal, pp. 262-8, will be found an account of the Persian Government lines, by Mr. Houtum Schindler, Inspector-General, Persian Government Telegraphs. As he has only *incidentally* referred to the lines erected and maintained in Persia by the English Government officials, it has occurred to me that a brief description of those lines, and the country through which they pass, may not be uninteresting to the Society.

To give a full and detailed history of the many political difficulties which had to be overcome before the Persian Government gave its consent to the erection of the telegraph from Bushahr to Khanikin, *via* Tehran, and to give an idea of the apparently insurmountable obstacles which were afterwards thrown in the way of those engaged on the work, would take up too much space, and be somewhat out of place in the pages of the Society's Journal; suffice it to say that all these difficulties were finally overcome by the moderation and tact of the different officers in charge, and that on the 13th October, 1864, the Director, Major (now Lieut.-Colonel) Champain, R.E., was able to report to the Governor of Bombay that the line of telegraph from Bushahr, on the Persian Gulf, to the Turko-Persian frontier at Khanikin, a length of 1,100 miles, had been completed. This is the origin of what is now called the Telegraph Department in Persia.

The first idea appears to have been that this line should act simply as an alternative one for keeping open the communication between Kurrachee and Europe, the more direct Mesopotamian line from Fao to Baghdad being wilfully damaged again and again by the Arabs.

Within a few days of the completion of the Bushahr-Tehran-Khanikin line, disagreements arose between the Persian and English authorities in Tehran, in consequence of which, interruptions were intentionally caused by the instruments being disconnected, and the line between Shiraz and the sea cut by the

villagers and wandering tribes, who, most probably, had received from Tehran a hint to act thus. The telegraph was in consequence so much damaged in many places that, when a few weeks later the difficulties were smoothed over, nearly all the line between Shiraz and Bushahr required re-erection, and this was only completed in January, 1865.

Colonel Stewart, the Director-in-Chief, who had worked so hard in furthering the scheme, both in Tehran and in Constantinople, unfortunately did not live to hear the welcome news that the Persian lines were open throughout, and working well. The death of this accomplished officer, who had endeared himself to one and all, was felt as a personal loss by the whole staff. One graceful compliment among many others has been recently paid to his memory, by naming after him the new cable-ship for the Persian Gulf section: this recognition of his valuable services has given sincere pleasure to all who knew him.

The line being now in working order, the Persian Department, as well as the English, was, according to a previous agreement, entitled to its use. Each Department worked alternate hours; but this system was found to be most inconvenient, as it gave rise to continual quarrels between our employés and the Persian clerks. It was therefore resolved that a second wire should be erected throughout the whole line, so that one of them might be put at the entire disposal of each nation.

After many tedious delays and heart-burnings, Colonel (now General Sir Frederick) Goldsmid, the then Director-in-Chief of the Indo-European Telegraph Department, was enabled to leave Tehran early in December, having concluded with the Persian Government a convention for the erection of a second wire, and the maintenance by an English staff of the whole line for a period of five years.

Acting under the convention, line stores of every kind were procured with all possible speed from England, but still the difficulties in collecting poles, labourers, and mules were such that many months elapsed before the work could be got under way. It is impossible for anyone who has never had to deal with Orientals to conceive the amount of *vis inertiæ* to be overcome in a Persian

official; but, happily, the same tact and discretion which carried the first line through were displayed in this second undertaking, so that by August, 1867, the Director, Major R. M. Smith, R.E., was in a position to announce that a second wire had been erected from Bushahr to Khanikin, and was working well.

The convention stipulated that one wire should be used exclusively by the Persian Government, and the other by the English staff, for Indo-European work.

In 1869, Messrs. Siemens commenced constructing their lines through Europe and the Caucasus, and they also obtained a concession from the Persian Government to erect and maintain in Persia a line of three wires from the Russo-Persian frontier to Tehran. This was completed and open for working by the end of January, 1870. The desirability was then felt of erecting a third wire from Tehran to Bushahr, to allow for the probable increase of the traffic, and to enable a more efficient control to be exercised.

No difficulties were experienced in this, the Persian local authorities having doubtless become used to our presence, and having discovered that to impede our progress by small interferences only led to a temporary delay, and not to a permanent stoppage of the work.

At this time it became evident that, as the Indo-European Company's lines were working well to England, there was no further need of an alternative wire from Tehran to the Turko-Persian frontier, and that the amount of traffic between Persia and Turkey in Asia would not justify the expense of keeping up an additional maintenance and signalling establishment. The line was thoroughly overhauled and handed over to the Persian Government officials.

Last year the Director made an inspection of this line, and he told me that, where in 1870 there were two wires, now only one was to be found, and he did not think that, out of the many thousand insulators which had been handed over, more than a thousand remained on the poles.

At the end of 1872, when the term of five years fixed by the convention would expire, it became necessary that a new arrangement should be made. To this end, in July, Major Champain

arrived in Tehran, to assist Mr. Ronald Thomson, the Chargé d'Affaires of our Legation, in negotiating the matter, and, eventually, these two gentlemen had the satisfaction of concluding, in spite of great and almost insurmountable difficulties, a most valuable political and commercial convention.

Since the signing of this new convention, everything has gone on smoothly up to the present time.

In giving this short sketch of the history of our Department, I have laboured under great disadvantages, having to trust almost entirely to memory, as no books or papers for reference were at my disposal.

A short description of the country which the line traverses will not be out of place here. I will ask you, therefore, to accompany me in a quick run from Bushahr to Tehran. As the line between the latter place and the Turkish frontier is no longer under our administration, I will not trouble you with an attempt to describe that part of the country. We will start from the office at Bushahr, or rather the new office at Reshire, now called Mukhbărabàd (which being translated means, the abode of the men giving news). This name was given partly as a compliment to the Persian Director-General of Telegraphs, the Mukhbar-ud-Dowlah, or the news-giver of the State, a perfect gentleman, and one to whom we are all much beholden for his unvarying courtesy, and also for his kindly aid in many difficulties.

Starting then from Mukhbărabàd, on horseback, a short run of about 4 miles nearly due east, across a rocky plain, brings us to the Masheelah, a sandy, salt marsh, generally more or less under water, without a stone or a weed of any description on it. After slipping, sliding, and ploughing through this sea of despond for about three hours, we pass and leave on our right the village of Chahgadak, and in two hours more arrive at Ahmedy. The ground is of the same sandy quality as the Masheelah, but as it is not flooded by the sea, progress over it is not much impeded, except during heavy rains. The village has a fine caravanserai, where we put up for the night, having come about 22 miles. Starting early next morning, we pass through a plain, the uniformity of which is only relieved by occasional clusters of date-palms, and

arrive, after about four hours ride, at the village of Khushâb, celebrated for the battle fought near it between the Persian troops and our own in 1857. Another two hours in the saddle brings us to the town of Borazjoon, where we come to the first telegraph office on the line, its distance from Bushahr being about 40 miles. This town and its neighbourhood used to contain the most lawless population in the country: they were a continual source of annoyance to our people by their breaking the line and shooting down the poles whenever they passed near them; but firm treatment on the part of the authorities generally has reduced them to comparatively good order. Borazjoon is not an enchanting spot. It is intensely hot in summer, being swept by continuous hot winds and sand storms: the water is brackish and disagreeable, drinking-water having to be brought a distance of 10 miles from the hills on donkeys.

From Borazjoon we proceed to Dàliki, about 14 miles: nearing the village we pass a number of naptha springs and small sulphurous streamlets. Just behind it the line enters the hills, and commences its ascent to the plateau of Persia, when, after numerous twists and turns, ups and downs, for about 6 miles, we reach, at a point situated about 800 feet above the sea, the brackish river of Dàliki. Near the bridge of the same name, the line crosses the river by a span of about 800 feet, and then, running across a range of hills, which confine the river, descends into a gorge and rejoins the road about 3 miles further on. Here both follow the same course, and ascend to the Konar Takhteh plain, 1,800 feet above the sea, by the Kotul Maloo, one of the worst bits of road in this part of the country. At a distance of about 4 miles beyond the top of the Kotul, and in the middle of the plain, is the village and telegraph office of Konar Takhteh, 70 miles from Bushahr. The first 4 miles of our next stage are soon passed, as the road is good and runs through the plain; a short stony ascent and descent then bring us to the Shahpur river, which we follow for about 2 miles. Leaving this, we enter into a long and intricate series of gorges, which lead us to the foot of the Kotul-i-Kamaridj, a sharp steep ascent over very bad ground, which brings us to the valley of the same name, 3,000 feet above the *sea.*

From the foot of the Maloo to the top of the Kamaridj Kotul, the line presents a most striking aspect, now plunging deep beneath our feet, then rising almost out of sight above our heads, and stretching in spans, varying from 200 to 500 yards, from peak to peak of the magnificent limestone hills, through which wind the footpaths which have been made for the use of the inspecting linemen.

Leaving the village of Kamaridj on our left, we cross the plain, in a straight run of 8 miles, to the foot of the Gatch hills; a short, sharp ascent brings us to the top, and a pleasant ride of 4 miles along the crest of the hills, clad with the wild almond and heath in full bloom, with a sharp drop at the finish, and we are again in a plain. Leaving on our left the ruins of Shahpur, so-called after the king of that name who founded the town and conquered the major portion of Western Asia in the third century, we ride for about 20 miles through a country covered by the most luxuriant herbage, dotted over with konar, a tree closely allied to the elder, and arrive at the town of Kāzeroon, 34 miles from Konar Takhteh, and about 104 from Bushahr. This town, which previous to 1871 had a population of 35,000 people, now only has at the very outside 10,000 inhabitants; having suffered as severely, if not more so, than any other place in Persia during the famine of 1871-72.

The general direction of the road has hitherto been north-east, but from here to Shiraz it changes to almost due east. Continuing to follow the plain of Kāzeroon, we reach, after 8 miles, the edge of a marsh and salt lake; another 3 miles over stony ground bring us to the foot of the Kotul-i-Dokhtar (pass of the maiden), which leads by a very steep and zigzag road to the plain of Dasht-i-Barm. The line ascends it in a single span, 2,100 feet horizontally, and 620 perpendiculary.

The Dasht-i-Barm, or plain of the pond, and the hills which surround it, are well covered with beautiful turf, wild flowers, and the Syrian oak. At its south-eastern extremity commences the long and difficult ascent of the Kotal-i-pir-i-Zan (old woman's pass).

Opposite a caravanserai there, the line crosses a deep ravine,

in a span of 2,950 feet long and 610 high, and ascends the summit of a hill towering above us. This was the original course of the line, but a more convenient and less dangerous route has lately been adopted, the line being taken up the face of the hill to the left; one wire of the old line remaining as an alternative line, in case of any accident to the main wires.

Again ascending, we reach, after an hour's hard work, the crest of the Pir-i-Zan, 7,250 feet above the sea. Again a steep drop of 800 feet brings us to the charming plain of Dasht Arjin (the plain of the wild almonds). It is completely closed in by mountains. From those on the right tumbles a magnificent waterfall from a perpendicular height of about 500 feet, and of which the water collects, forming a marsh and lake, which teem with water-fowl, wild pigs, etc.; but the valley possesses, also, the unenviable notoriety of being a favourite haunt of the lion. Accidents have happened to various members of our Department: a carpenter was attacked near it, killed, and devoured; a messenger met with a similar fate, and a lineman had his horse killed. Major St. John, when riding quite alone after sundown, was attacked, but managed to slip off his horse, which ran off pursued by the lion, and, having got rid of it, was brought into the village the next day.

The spoor of the lion is constantly seen about this spot, so that the linemen are rather afraid of going out alone, which they have, however, to do when on interruption duty.

On the northern side of the plain is the village and station of Dashtargen, 6,600 feet above the sea. Having left the station, we pass over the Seenah-i-Jefēd (white breast), a hill composed entirely of gypsum, and descend to the bed of the Kara-Aghāch (black-wood) river, known in the relation of Nearchos's journey by the name of Sétakos; and following for 6 miles the course of the stream through undulating country, we arrive at the caravanserai of Khaneh Zenīan, 6,100 feet above the sea.

For the first 10 miles of the next march the country is open and undulating, and, being so elevated, the snow lies very deep there during the winter months. Five years ago the late Superintendent of this Division was snowed up in the caravanserai for four weeks, and could neither get back nor forward.

About 8 miles from this place Sergeant Collins, of the Royal Engineers, Inspector in charge of this section, was murdered. Soon after passing the spot where this sad event occurred, we enter a succession of gorges, and after about 12 miles of gradual descent, the blue domes of the Imámzádehs (tombs of saints), the minarets of the mosques, and the numerous cypress trees which from a distance make Shiraz look like an enormous churchyard, meet our eye, and far away on the distant plain glitters the salt lake of Mahatu. A short two hours and we enter the town itself, 185 miles from Bushahr, and 5,200 feet above the sea.

Hitherto, we have been travelling what is called "caravan," but now we can proceed "cháppár" (*i.e.*, by post). Between Bushahr and Shiraz post horses are not laid out, and the country is indeed so broken that it scarcely admits of this being done.

From Shiraz the line runs almost due north-east through a narrow gorge in the hills, called the Tang-e-Allah Akbar, passing beneath a gateway where, in a room above, is kept one of the oldest Koráns that is known to exist. The road lies through a mass of low hills, which, after about 16 miles, open out into the Zergán plain, the village of that name being our first stage. Changing horses we reach, after 7 miles ride over even ground, the river of Kur, or Bandamir, probably best known to most of us through the verses in Moore's "Lalla Rookh," and cross it near its junction with the Pulvár, by the Pul-i-Khán (bridge of the khan). Just before arriving at the spot where the river Pulvár enters the plain, we see on our right, about 2 miles off, under some hills, the stupendous ruins of Persepolis,—the creations, as we are told by the cuneiform inscriptions on the walls, of Darius and Xerxes; destroyed by Alexander after a debauch,—and, in spite of twenty-three centuries, the bas-reliefs which remain are, many of them, as perfect as on the day they were sculptured. North-west of the Chappar Khaneh (post-house), about 2 miles off, in a precipitous range of hills, are the tombs of Darius and three other Achemenian kings, cut out of the live rock: with a glass, the carvings and bas-relief which ornament their façades are distinctly seen.

In the plain of Mashed-e-Morghab, we again meet with remains of the past, the most conspicuous, the tomb of Cyrus, being a

curious pyramidal-shaped building. A few other ruins are scattered over the plain, all that is left of the once famous town of Pasargadæ, which was founded by Cyrus on the battlefield where he conquered Astyages, and won for the hitherto unknown tribe of the Persians the Empire of Western Asia. On several pilasters is engraved, in cuneiform characters, this pithy inscription: "I am Cyrus the King, the Achemenian."

From here the line takes a course due north. Soon after leaving the post-house we enter a range of limestone hills, and arrive, by a stony road, at Khaneh Kergun, 14 miles; then across a dreary bleak plain we reach Dehbeed, 14 miles, where only the telegraph office, post-house, caravanserai, and a ruined palace of a bygone king of Persia, are to be seen. Dehbeed is situated at about 7,700 feet above the sea: for many months of the year the plain is completely covered with snow, and ice has been known to form here as early as the 24th of August. About 8 miles more of this plain and we again enter the hills, another 10 miles bringing us out near the Chappar Khaneh of Khaneh Khorreh, in all 18 miles, a dismal spot; hence to Surmah, 26 miles, and to Abadeh, 14 more, is a gradual descent, the intervening country being quite barren and uninhabited. Abadeh is about 6,450 feet above the sea: it is surrounded by a number of villages, the country all around being well cultivated. From Abadeh to Shulgastan, and thence to Yezd-i-Khast, stages of 18 and 25 miles, is again a sterile uncultivated plain. Yezd-i-Khast is a curious town, built on a sort of rocky island, in a large gully, cut out of the plain by some antediluvian river. The line crosses the gully in a span of about 400 yards. From Yezdikhast to Maksudbeggi, 22 miles, is the same uninterrupted plain, and thence to the town of Komishah, 15 miles, the road goes through a succession of villages and cultivated ground. Between here and the half-ruined village of Mayar, 18 miles distant, the country presents the same uniform aspect. Thence for 16 miles, through a level plain, with vestiges of past prosperity, and on through a gorge and down a small steep pass called Urchini (the steps), brings us to the post-house of Marg, distance 25 miles, whence we can perceive the outskirts of Ispahan. By an easy ascent we reach the plateau of Hazar Derreh

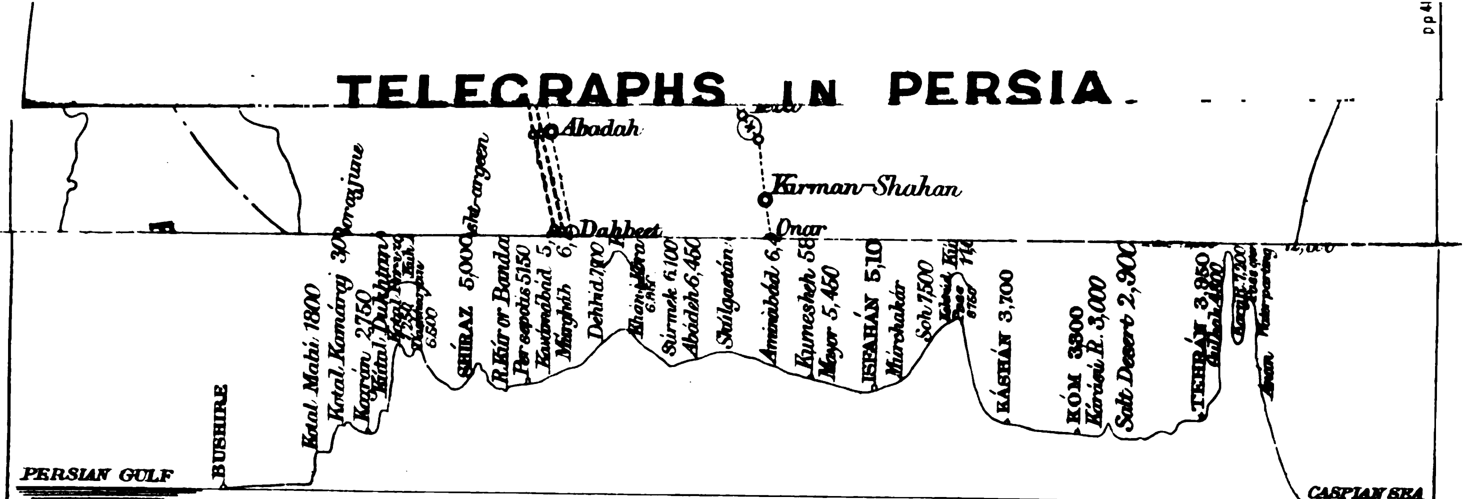
(thousand ravines), and there we have the whole town and plain of Ispahan lying below our feet.

This first *coup d'œil* of Ispahan, especially in the spring, is very grand. The town stretches for many miles on both sides of a magnificent river, spanned by several fine bridges; the many coloured domes, the minarets of the mosques, the groves of fine old chenar trees (oriental plane), the numerous queer-shaped pigeon towers, the quaint and characteristic forms of the hills near the town beyond it the cheerful aspect of well cultivated fields, merging into swelling plains, which stretch up to the foot of the snow-clad mountains of Kohrud, the whole forming an almost unequalled panorama.

Passing through the graveyard of the Armenians, thence by the ruins of what but lately was a fine palace, we arrive at the Armenian quarter of the town, called Julfa, distance 10 miles. The line passes through this, and crosses the river on one of the bridges, thence through the town and out of it by the Tehran gate, and so on, to Guz, 10 miles, whence by gradually rising ground to Murchekor, 25 miles, thence we enter the Kohrud hills at the village and station of Soh, having previously changed horses at a village called Bedeshk. Soh is a pleasant little village in the midst of the hills, about 7,500 feet above the sea: about 10 miles beyond it we cross, by a pass of no great difficulty, the watershed of the range, at a height of about 9,500 feet, being surrounded by peaks attaining a height of 12,000 feet; we then descend by a fairly easy gradient to the village of Kohrud, one of the prettiest spots on the line.

From this village we follow the stream of Kohrud by its gorge into the plains, over a stony slope, to the town of Kashan, 15 miles distant.

We now proceed along the edge of the desert, and with the Kohrud hills on our left, to Sinsin, 23 miles, and thence to Passangan, 27 miles, and to Kom, 14 more, the aspect of the country being somewhat better near the town of Kom, being even well cultivated. This town is celebrated for its holy shrine, with its gilt dome, and many Persians are brought from long distances to be buried within its sacred precincts.



Section between the Persian Gulf & the Caspian along the high road through Bushire Shiraz Isfahán & Tehrán.

Horizontal Scale 60 Miles = 1/2 Inch.
Vertical Scale 4000 Feet = 1/2 Inch.

Passing right through the town for about 1½ miles, we again emerge on the plain, crossing the river by means of a fair bridge.

To Pul-i-Dellak, 14 miles, the country is all a desert: near that station we again cross a sluggish stream by a bridge. Four miles after leaving the post-house, we pass the low hills of Sadrabad, and the caravanserai of the same name. Here we now come upon an arm of the Great Salt Desert of Persia; 16 miles brings us to the caravanserai and post-house of Houz-i-Sultan; about 8 miles further on we decend into the Valley of the Shadow of the Angel of Death, well deserving of its name, for a more dreary, weird spot it is impossible to imagine.

After riding through this desolate valley for about 8 miles, we cross a dull brackish stream; an ascent of a few hundred feet shortly after brings us into the well-watered little plain of Kenáregird, 25 miles; a gradual rise, and some small hill work, and we pass the crest of the Kenáregird hills, and thence survey the plains of Tehran, with the capital in the distance; behind it the plain of Shemeran, with its villages and divers palaces of the Shah, the whole being backed by the before-mentioned mountains of Elburz. So, gradually descending, passing villages, skirting gardens gay with kiosks and houses of various descriptions, we enter the town through the Kazvin Gate, and finally reach the European quarter, situated on its northern side.

The line originally consisted of one wire, No. 5 B.W.G., erected on wooden poles, with the exception of 300 iron poles of Siemens' pattern, and 250 Hamiltons, used near Bushahr and was suspended from iron-hooded Siemens's insulators. The second wire, put up in 1867, was of a similar kind, and was suspended on insulators of the same pattern, from the already existing poles. As in certain parts of the country white ants and dry rot rapidly destroyed the poles, it was deemed advisable to procure iron ones, and subsequently 2,000 of the kind known as Siemens's pattern were sent out and erected in the most affected portions of the line. Since then, iron poles of the same type have been occasionally sent out in batches of 1,000; so that at present the three wires, each of

No. 5 B.W.G., of which the line now consists, are throughout supported by iron poles. On the plains the poles are erected 80 yards apart, but in the hills the distance is adapted to the features of the ground. The poles are of two kinds, known as strainers and intermediates; about every eighth one is a strainer, the former being of much stouter build than the latter. A Siemens's pole consists of four pieces—the foot-plate, a buckled plate of sheet-iron, to which is bolted a cast-iron socket, into which again a wrought-iron upper tube is cemented; on the top is placed a lightning conductor. The total height of the poles used in Persia is about 17 feet 6 inches, and they stand, when in the ground, about 14 feet 6 inches out, with, as a rule, the lower wire 11 feet clear. The insulators used, of the type previously mentioned, are secured to the poles by two bolts and a shackle. In some cases the two upper insulators are bolted together: the wire is simply suspended by the intermediate insulators in a hook, coming from the porcelain bell; but the straining insulators have a stalk which has two slots in it resembling a double ⊥; the wire is drawn through one of these slots, strained and regulated, and then secured by an iron wedge, which is hammered home; a turn being then taken in the wire, it is put in the opposite slot and again wedged, a loop being left of about 18 inches in circumference. The object of this loop is to procure a reserve of wire for regulating or for making a joint in case of a break. In the last batch of insulators sent out by Messrs. Siemens for these lines, a modification in the straining insulator has been introduced, and in place of the wedge a cam is used; but this has not proved quite successful, as the cam almost invariably springs whenever a strain is brought to bear upon it. For angle poles, stays are always used, either of one, two, or three wires, as circumstances dictate. I am told on the Indo-European Company's lines they use a second pole as a strut.

The staff of the Department consists of a Director, 2 Superintendents, 1 Medical, and 2 Medical Assistant-Superintendents, 6 Inspectors, and 22 Clerks. Four of the inspectors and eight of the clerks are non-commissioned officers of the Royal Engineers.

The whole line is divided into two divisions, each under a

Superintendent, each division being sub-divided into three sections under an Inspector. The Assistant-Superintendents have charge of a main station, and are subordinate to the Superintendent of the division.

The 1st Division extends from Tehran to Abadeh, with head-quarters at Tehran.

1st Section: Tehran to Kashan, 480 miles of wire.
2nd Section: Kashan to Ispahan, 348 miles of wire.
3rd Section: Ispahan to Abadeh, 420 miles of wire.

2nd Division.—Head-quarters, Shiraz: Abadeh to Bushahr.

1st Section: Abadeh to Shiraz, 540 miles of wire.
2nd Section: Shiraz to Kazeroon, 255 miles of wire.
3rd Section: Kazeroon to Bushahr, 363 miles of wire.

STATIONS IN PERSIA.

Station.	Distance in Miles from Station to Station.	Height in Feet above sea.	Staff at each Station.
Tehran...	82	3,950	Assistant-Superintendent and 5 Clerks.
Kōm	60	3,300	1 Clerk, 1 Inspector.
Kashan...	43	3,700	1 Clerk.
Soh	60	7,500	1 Inspector.
Ispahan	50	5,100	Assistant-Superintendent and 3 Clerks.
Komeshah	78	5,800	1 Inspector.
Abadeh..	58	6,450	1 Clerk.
Dehbeed	58	7,700	1 Clerk.
Sivend	54	5,500	1 Inspector.
Shiraz	40	5,200	Assistant-Superintendent and 5 Clerks.
Dashtarjin	32	6,500	1 Inspector.
Kazeroon	34	2,750	1 Clerk.
Koanr Takhteh ...	28	1,800	1 Clerk.
Borazjoon	40	200	1 Clerk.
Bushahr	...	...	1 Inspector.

Tehran is the terminal station, where the lines of the Department meet those of the Indo-European Telegraph Company. The same instrument-room is used by both services, and the messages are passed across from one instrument to the other.

Ispahan and Shiraz are translation stations, the others being for control. At Bushahr there used to be a station belonging to this Department, but for economical and other reasons it was amalgamated with the office of the Persian Gulf Department: it now forms part of the latter administration.

For the maintenance of the lines, each Inspector has under his orders a head lineman and four subordinate linemen; they are all natives. Their duties consist in going out and patrolling the lines at stated times, and they have to hold themselves in readiness to proceed out on interruption duty at any hour: they have to provide and keep their own horses. In the second division there are also a certain number of Tofangchis (guards): they are stationed at intervals along the line, and have also to patrol it, reporting to the nearest station in case any damage should have been done to the lines, and noting what tribes are encamped about the line or have passed near it.

The lines are tested for insulation and conductivity at 9 o'clock each morning with a Wheatstone's bridge at the main stations. The operation is conducted in the following manner—Tehran, at 1 minute before 9, gives a time beat, which is taken off at 9 exactly; Ispahan, immediately on the beat being taken off, insulates all wires on the Tehran side for 2½ minutes, and loops them for the next 5 minutes. Tehran makes his tests, Shiraz doing the same, whilst Ispahan makes his tests, and Bushahr for Shiraz. On the expiration of the 7½ minutes, the operation is reversed along the whole line. These tests are all recorded. At 5 minutes to 6 a.m. the main stations communicate on all wires. The control stations come in on the third or control wire, and call the nearest main station; they are then informed of the state of the lines. Should there be any faults existing, the two stations each side of the fault immediately send out their linemen, who proceed under the line to the locality of the fault, and repair it with the least possible delay.

At 9.30 the control stations have to call up the nearest main station to receive telegrams, orders, etc. Again at 2 p.m. and 5 p.m., and at 9 p.m., to receive permission to close for the night.

During the day, that is, from 9.30 a.m. till 5 p.m., the whole

of the stations are in circuit, so that in case of interruption they can be called up and the fault localised with a minimum amount of delay. Whenever a total interruption occurs, it is the duty of the Inspector in charge of the section in which the lines are interrupted to proceed as fast as possible to the fault, and to restore communication. Each Inspector is provided with a light sounder and portable battery, either of the Marie Davy, or Mallock type: they are thus enabled to open out and communicate with the station on either side.

During the year 1877-78 there was sixty-eight interruptions of various descriptions throughout the line, lasting in the aggregate 27 days 20 hours 38 minutes, and divided as follows:—

Interruption of one wire, not stopping traffic: 52 faults, lasting 497 hours 21 minutes.
Interruption of two wires, not stopping traffic: 11 faults, lasting 113 hours 52 minutes.
Interruption of all wires, stopping traffic: 5 faults, lasting 57 hours 25 minutes.

The causes which produce interruptions are various, and are rarely due to faults in construction: for instance, on two occasions a village through which the line passed was maliciously set on fire. Heavy gales have blown poles out of the ground on the Bushahr plain, and also in the Great Salt Desert in the north, where, although deeply set, they have little hold on the ground. During severe snowstorms, the snow getting frozen on the wires often breaks them down, and even at times by the excessive weight absolutely breaking the poles. The very last case was one of this kind, the cast-iron socket breaking within a foot of the ground. On the other hand, we have known cases where the wires have been embedded for yards in what I may call dry snow, having a very high resistance, through which we were enabled to work with comparative ease. Of total interruptions we are happily fairly free, when the length of the line, the features of the country, and especially the severity of the weather during the winter months are taken into consideration; but the interruptions of one or two wires by contact are somewhat frequent.

Although contacts are often wilfully made by the inhabitants of villages near to the line, they are more frequently occasioned by the breaking of a wire near a straining insulator. The reason of this is, beyond doubt, a defect in the system. It has already been explained that the wire is secured at every eighth or tenth pole by a straining insulator: the mere fact of hammering the wedge home to secure the wire hardens a certain portion of it, and reduces considerably its co-efficient of elongation. For a country like Persia, subject to variations of temperature, ranging from 150° or 160° Farenheit to below zero, the wires require frequent regulation: each time this is done a fresh piece of wire is hardened; when, therefore, they are subjected to any untoward strain, a breakdown is the immediate result. Another great inconvenience, inherent to this system, is that the broken wire, springing back and running out of three or four insulators, in all probability gets itself interlapped with the other wires, and so produces a contact of all wires.

Binding the wire into every insulator has been tried, but the stalk of the intermediate insulater is so small, that it does not admit of a firm grip being caught, and thus the binding wire generally breaks away.

Another point in the system of securing by wedging is worthy of consideration. The mere fact of hammering home the wedge is likely to crack the porcelain bell, and in damp weather to afford a good point for leakage.

The substitution of a cam for a wedge appears to me to be a step in advance.

Among the sources of annoyance devised by the natives, hitherto the most frequent and disastrous is that of pole shooting. Happily, of late years, in consequence of the very stringent measures taken by the Prince Governor of Fars, it is decidedly on the decrease. We have no idea of the object of this shooting, but it seems to be peculiar to the nomad tribes of this part of the world. The same sort of thing goes on in Makran. The gun is apparently fired close to the pole; the bullet makes a round hole where it enters the socket, but if it has sufficient penetration, it

knocks away on the other side a piece as big as one's hand, completely incapacitating the pole for further work.

The wrought-iron poles, generally known as the Hamilton, of which we have some few in use, stand this rough usage very much better than the Siemens: the bullet passes completely through them, making only a small hole on either side. There are some poles near Bushahr riddled with over sixty such holes, and yet as strong and as efficient as the day they were put up.

It would be impossible to give any idea of the amount of damage done to the section of the line between Bushahr and Kazeroon, and again, in a lesser degree, between Kazeroon and Shiraz, except by figures. In the year

1874-75	... ...	213	poles were bulleted;
1875-76	... ...	88	,, ,,
1876-77	... ...	45	,, ,,
1877-78	... ...	25	,, ,,
1878-79	... ...	21	,, ,,

therefore, in a section of over 1,700 poles, within the last five years, 392 have been bulleted, or nearly 25 per cent., equal to an average of 5 per cent. per annum. To prevent the sockets from being bulleted, whenever practical, a cairn has been built up with stones to where the standard joins the socket: this has been found efficacious, as the natives do not take the trouble to pull the cairns down in order to shoot at the socket, but fire at the standard, which, being of wrought-iron, and fairly thick at this point, offers a good resistance to the bullet; even should the bullet enter, as it sometimes does, it does little or no harm.

Besides the bulleting, we have not unfrequent cases of sockets cracking, generally at the junction of the socket and standard. What causes this has been a fair puzzle to us all, but we have come to the conclusion that it must be brought about in the manufacture, probably by the castings being allowed to cool too rapidly.

Poles that have been up for years, on inspection, will be found cracked; others, that have only just been put up, will crack immediately. Situation appears to have nothing to do with it, as they are found to crack equally on the high plateau and near the

level of the sea. As it is too expensive to replace a bulleted or cracked pole by a new one, one entire pole is made to do duty in repairing two broken ones. In the first case the new socket is put in, and the pole is again made ship-shape. In the second case, the old socket is smashed level with the ground; in this is cemented a standard, with its top served with wire, and over this a second standard is placed: thus a somewhat neat and most effective pole is made. A pole of this description never gives any trouble.

I cannot but think that, for such countries as Persia, poles made with a short cast-iron socket, coming only up to the ground line, with a lower and an upper wrought-iron standard, made generally after the Siemens pattern, would be most efficient.

The Hamilton poles used in Persia have not the same capability of standing a lateral strain as those of the Siemens pattern.

In many cases they have been known to buckle when the wire has been strained.

Owing to the general excessive dryness of the air in the uplands of Persia during the months of April to November, the insulation of the lines is exceedingly good, averaging about 350 megohms per mile.

As, however, there is at the end near Tehran the salt desert, from which particles of salt are being continually blown about and deposited on the insulators, whenever there is any rain the insulation immediately falls to about one megohm, and remains at that value until the weather clears up again.

At the Bushahr end we have the Masheelah, and also some 40 miles of plain impregnated with salt.

Besides the natural dews, incidental to the situation, the plains bordering on the sea are sometimes swept by the densest fogs, in comparison to which a London fog is as nothing. The insulation on this portion of the line has always been abnormally low, partially owing to defective insulation, and to the fact that, at certain times of the year, masses of spider webs, which nothing but daily cleaning could keep down, cover the insulators and wires. I have seen streamers of the web, like a fine silk thread, connect the wires with the ground.

However, as soon as the rains set in, these are all swept away,

but, should a fog precede the rain, the leakage is so great that the line for some hours is practically broken down. This has occurred but once or twice. We, however, always experienced a certain amount of difficulty in working during foggy weather. Some measure was evidently necessary to put the line in a somewhat better state of insulation, and the whole section was therefore carefully gone over, and on No. 2 wire (the main working one) every defective insulator was removed, and replaced by one that had been thoroughly tested before being sent out on the line. The amount of spare insulators in store would not allow of the same course being taken with No. 1 and No. 3 wires. Of 191 insulators tested in Shiraz, 25, or 13°/₀, were found defective and useless. A large number were also tested at Bushahr, but no record has been kept of the result. However, all defective insulators of No. 2 wire between Kazeroon and Bushahr were replaced by good ones, the number amounting to 429, or 25°/₀. Immediately before the work was commenced, the insulation test of the Bushahr-Borazjoon section in wet foggy weather was only 3,147ω, or 125,880ω per mile; whereas on completion of the work, in similar weather, it gave 20,100ω, or 804,000ω per mile. To-day, the weather being fine and abnormally dry, the same wire gives 2,040,000ω, while the other wires only give 430,000ω per mile. The usual insulation of the Shiraz-Bushahr section in summer is about 90 megohms, and in winter about 2 megohms; sometimes, however, it falls as low as 0·5 megohms. To exemplify to what degree the damp of the plains near Bushahr affects the insulation, I give a series of tests which were made at Konar Takhteh station, first with Borazjoon, 28 miles distant, and then with Kazerun, 34 miles away.

KAZEROON—KONAR TAKHTEH.

Date.	Time.	Temp. F.	State of Weather.	Insulation.		
				No. 1.	No. 2.	No. 3.
1878 Jan. 1st	a.m. 6.30	58°	Dew, Konar Takhteh end	7,000	4,900	13,000
" 6th	"	58°	Cloudy	7,100	3,200	15,000
" 10th	6.25	61°	Foggy	2,180	1,250	7,500
" 11th	"	57°	"	3,900	1,870	8,400
" 15th	6.30	60°	Dew	4,400	2,600	9,900
" 21st	"	62°	Dew, Kazeroon end ...	10,900	9,500	20,000
" 22nd	"	63°	Heavy rain	8,000	5,000	17,000
" 23rd	6.35	59°	Fine	15,000	12,000	22,000

KONAR TAKHTEH—BORAZJOON.

Date.	Time.	Temp. F.	State of Weather.	Insulation.		
				No. 1.	No. 2.	No. 3.
1878 Jan. 1st	a.m. 6.40	58°	Slight dew	4,700	2,800	7,900
" 6th	"	58°	Cloudy	910	550	2,560
" 10th	"	61°	Fog at Konar Takhteh ...	810	484	2,050
" 11th	"	57°	{ Fog, Konar Takhteh, Clear Borazjoon ... }	160	400	1,800
" 15th	7.5	60°	Dew	2,450	1,430	5,030
" 21st	6.40	62°	Slight dew	3,790	2,320	6,350
" 22nd	"	63°	Heavy rain	3,540	2,340	6,800
" 23rd	"	59°	Dew, Borazjoon end ...	3,250	2,100	5,100

Remarks.—11th, half an hour after went up to 320; at 10 a.m., 1,420; at 2 p.m., No. 1, 2,070; No. 2, 1,340.

It will be noticed that No. 3 wire invariably gives the highest results, which is to be accounted for by its being the wire last erected, and therefore having more perfect insulators.

For the section Bushahr to Konar Takhteh, we are to have, sooner or later, some of Messrs. Johnson and Phillip's new insulators, so as to thoroughly re-insulate one wire at least. It would be difficult to find a section of line that would more thoroughly test the powers of any insulators.

If this insulator does its work in this situation, there will be but little doubt of its being a thoroughly good one.

Up to the year 1877, the only instruments used on these lines were Siemens's Single Current Morse inkers, with polarised relay; it was, however, felt that some better system was necessary, and it having been seen how well the Double Current system of the same makers worked on the lines of the Indo-European Company, instruments of the same pattern were indented for from England to work the international wire.

As a very full description of these instruments was given by Mr. Gustav Risch in a paper read before the Society, which will be found in No. XIX., Vol. VI., p. 284, of the Society's Journal, it is needless for me again to describe them.

The instruments sent out to this Department differed somewhat in the mechanical arrangements from those used by the Indo-European Company, modifications having been introduced in the automatic starting and translating gear, and a special arrangement being used with a view to automatic working. The result has been most untoward: the instruments are constantly breaking down, and always, of course, at the busiest moments of the day; they require continual watching and regulation. To prevent needless delay to international work, the lines, whenever a breakdown occurs, are shifted over to a single current set of translators. After endless trouble and worry at Tehran, Ispahan, and Shiraz, the double current instruments have at last, after nearly two years, been got into fairly satisfactory working order, but at Bushahr they have been abandoned altogether, a double current key being fitted to an ordinary single current instrument.

I have, however, but little hesitation in saying that the results in the general working have quite justified the introduction of the double current system. The ill effects of the leakage previously referred to having been reduced to a minimum, complaints on this head are now unheard of.

Owing to the excessive dryness of the atmosphere during eight months of the year, static electricity is very apparent, although not to the great extent as described in Canada.

The effects of induction are seen here in full force: the induced

current is generally so strong, that by a fine adjustment of the relay of an instrument connected to another wire, every signal can be obtained as legible and clearly defined as on the primary wire. At Ispahan and Shiraz I have often read the whole of the work passing on the international wire when it has been direct through the station.

When the Indo-European Telegraph Company's lines were first erected, the two upper insulators were bolted together, but it was found that to work the two lines simultaneously between Tehran and Tiflis, 700 miles, was impossible, owing to induced currents, and the wires were therefore separated and placed at a fair distance apart, when no difficulty was experienced.

During the long interruption of the Eastern Telegraph Company's line in 1874-5, the whole of the work passed over these lines, and to get through the extra work two wires had always to be employed, and sometimes even three. When we first started the third wire, signals came through in a very broken and scarcely legible manner, and on experiment it was found that induced currents were the cause. By crossing the wires, however, diminishing the battery power of the two main circuits, and using on the third circuit a double current instrument, we were able to get the work through without difficulty.

Earth currents but rarely affect the working. During eleven years' experience I have only seen upon some three or four occasions currents of such amplitude as to necessitate steps being taken to remedy them; nor do I believe that earth currents are ever prevalent in our lines to any great degree, as I have tried at times to detect them, but have not met with general success.

For both line and local circuits the Minotto cell is used throughout the service: it is cheap and effective, the materials all being procurable in the country. A carefully made up battery for line work, even at the busiest station, will last a year.

A most effective cell has been adapted for local circuit. In place of the sawdust diaphragm, the old Morse tape is broken up and well steeped in water for some 5 days, and is then packed into the glass in a similar way to the sawdust: the cell comes almost

into immediate action, and in a few days works down to a low internal resistance, about 4 ohms. One of these cells will last three months. The idea has been borrowed from the Gulf Section, where these cells have now been used for some time.

In the paper of Mr. Risch, previously referred to, a considerable stress has been laid on the speed of working through to India during the year 1876. The distance given between Tehran and Kurrachee is 1,190 miles, whereas it should be 1,850 about, 1,050 being the length of the submarine line from Bushire to Kurrachee.

All attention is given to getting the international work through with the utmost possible despatch. The following tables, giving the average time taken to transmit messages between Tehran and Bushahr during the years 1876-77 and 1877-78, will show that no needless delays have occurred on these lines :—

TEHRAN-BUSHAHR SECTION.

Showing the average rate of transmission of messages in their transit through the lines between Tehran and Bushahr for 1877-78, as compared with that of the previous year.

Month.	1877-78. Average Time of Transit between Tehran and Bushahr. Distance 792 Miles.			Remarks.
	Hours.	Minutes.	Seconds.	
April, 1877	0	7	21	
May, "	0	8	2	
June, "	0	0	0	Total interruption of Company's lines.
July, "	0	0	0	
August, "	0	4	26	
September, "	0	4	15	
October, "	0	6	4	
November, "	0	21	11	Bad working; messages retransmitted.
December, "	0	7	57	
January, 1878	0	5	6	
February, "	0	3	25	
March, "	0	4	7	
Monthly average during the Year	0	7	11	

Month.	1876-77. Average Time of Transit between Tehran and Bushahr. Distance, 792 Miles.			Remarks.
	Hours.	Minutes.	Seconds.	
April, 1876	0	9	27	
May, "	0	7	0	
June, "	0	5	59	
July, "	0	6	38	
August, "	0	7	45	
September, "	0	7	30	
October, "	0	7	1	
November, "	0	11	46	
December, "	0	10	6	
January, 1877, ...	0	12	35	
February, "	0	7	57	
March, "	0	8	36	
Monthly Average during the Year	0	8	6	

When in 1872-3 the absolute practicability of working duplex was established, many young telegraphists turned their attention to finding out a system simpler and cheaper than that in general use. Amongst others, Mr. Fahie, Assistant-Superintendent in this Department, devised such a method, practical and simple to a degree. It is founded on the system of testing for internal resistance of batteries discovered by Mr. Mance. Mr. Fahie's duplex system will be found fully described in the Society's Journal, page 470, Vol. V. We have worked this system experimentally with great success between Tehran and Shiraz up to a rate of 35 words a minute; occasion has not, however, yet arisen to cause us to use it in practice.

It is to be regretted that Mr. Fahie never got the *kudos* for this simple invention that he deserved. In 1874 he sent home to the Secretary of the Society a paper fully describing the results which he had attained, but unfortunately his paper was lost on the way. Some considerable time elapsed before the mishap was discovered and the loss repaired, and thus time was afforded to others to forestall him in priority of invention.

What the future of telegraphs in Persia may be, as viewed by their utility to the general public, is a very problematical question. Although the Persian Government are year by year extending their system, yet the maintenance of their lines is so bad that, as portions of future international lines, they cannot be reckoned on; and any lines projected to this end would have to be erected and maintained by an European staff.

Now that the Afghan outbreak has subsided, and a line to Cabul is to be erected, reference is constantly being made to the advisability of connecting the north of Persia, *via* Meshad, Herat, and Cabul, with India, and thus forming a fifth telegraphic route between England and the East. It is perfectly true that a line does exist between Tehran and Meshad, but it can scarcely be counted as a link in the proposed chain. Although erected under European supervision, it was placed, when completed, into the hands of Persian officials, and has rapidly deteriorated. An English gentleman who was travelling in Khorassan lately, and who stayed at Meshad for some time, told me that this line was open on an average only two days a week, and that when he was returning to Tehran he saw at least two miles of wire on the ground. He informed the Persian official at the next station of the fact, but the information was received in a very casual way, as if it were of little or no consequence. The same story applies to all the lines of the Persian Government system, when left to the supervision of the natives. The two or three Europeans in the employ of the Telegraph Department are continually moving about the country putting the lines in order, but as soon as their backs are turned the lines are again allowed to take care of themselves.

To complete this route, a line from Tehran to Cabul, joining up to the projected line, would therefore have to be erected and maintained by an European staff. The total length would be about 1,200 miles.

The major portion of the country from Shahrud to Herat is being continually raided by Turcomans: the line could only be put up under a large guard; and inspection would be paid for by the captivity or lives of some of the staff. Carriage of material would be extremely costly, as everything would have to be transported

many hundreds of miles from the coast on mules or camels. Such a line would be enormously expensive, and, taking into account the competition it would meet with from the existing lines, could never pay even its working expenses. Nothing but political necessity would seem to justify its erection.

A line *via* Ispahan, Yezd, Kirman, and thence to Bundar Abbas, thence to Gwadur, was projected in 1865-66, but from obstacles thrown in the way by the Persian Government it was abandoned, and a second cable, insulated by Hooper's core, was substituted and laid between Bushahr and Jask, the latter place being connected with Gwadur by a land line of two wires. Had the original scheme been carried out, it would have had a great influence on our future in Persia: not only would the line have passed through two towns, Yezd and Kirman, of some commercial importance, having a large trade with Central Asia and India, whence a certain amount of traffic could have been drawn, but it would have possessed certain political advantages.

It is to be feared that the original cable laid in 1864 already gives premonitory signs of a not far distant dissolution: if these fears should unfortunately prove true, steps will have to be taken to provide a substitute.

Supposing the 1864 cable between Jask and Bushahr deteriorated beyond redemption, the problem to be solved will be, what steps should be taken to remedy the evil. The choice lies between a fresh cable and a land line to some point in the Persian Gulf joining the existing lines in Persia and those in the Gulf.

To carry out the latter scheme there are five separate routes to choose from, viz. :—

1. From Ispahan, *via* Yezd, Kirman Bam, Bampur, to Gwadur.
2. From Ispahan, *via* Yezd, Kirman, to Bundar Abbas, distance 750 miles.
3. From Abadeh, *via* Aberkut, Anderun, Kirman, to Bundar Abbas, 600 miles.
4. From Shiraz, *via* Darab, Forg Tarun, to Bundar Abbas, 320 miles.

5. From Shiraz, *via* Jarum Lar, to Bundar Abbas, 300 miles.

Bundar Abbas would have to be connected with Jask either by cable, 120 miles, or land line along the coast, 180 miles.

Of all these routes, the fourth, namely, from Shiraz, *via* Darab, Forg Tarun, to Bundar Abbas, is the one which presents itself to me as being the best. A great part of the country is open, and where there are mountains they are not of a character to present the same difficulty as those we have already encountered. This route, although somewhat longer than that of No. 5, is the one usually followed by caravans passing between Bundar Abbas and Shiraz, water being more easily found, and as it lies somewhat further to the north is therefore cooler. Sir Frederick Goldsmid, who took an immense interest in this alternative land line, and who knew the subject *au fond*, writes on the question as follows: "It is in fact a question well worthy of close consideration whether any additional wire now supplied to existing communication, or even any future third wire between Shiraz and Bushahr, would not be best provided for by a wholly new line from Shiraz to Bundar Abbas. Bushahr, having long been the usual port of traffic with the interior of Persia for British Indian vessels, has reaped the honours and advantages of a recognised line of communication, but this state of things could never have originated from geographical position or natural superiority; it is one of those chance contingencies that take root in the East, irrespective of British interests and British advisers. As regards India, Bundar Abbas is by sea 425 miles nearer to Bombay than Bushahr, and the land distance from the former port to Shiraz is only 130 miles more than from the latter; and while it is 1,330 miles from Bombay to Shiraz *via* Bundar Abbas, it is 1,625 *via* Bushahr."

There is but little doubt that at first wilful interruptions would be somewhat frequent, but the same energy and patience which have brought the existing lines to their present efficient state would soon have the desired effect, and a thoroughly workable and reliable line between Shiraz and Bundar Abbas would very soon be established.

List of Interruptions to Through Traffic on the Indo-European (Tehran) Route in 1877-78.

Month.	Indo-European Company's Section.	Indo-European Departmental Lines. In Persia.	Indo-European Departmental Lines. In Persian Gulf.	Total.	Remarks.
1877.	D. H. M.	D. H. M.	D. H. M.	D. H. M.	
April	0 1 9	...	...	0 1 9	
May	21 1 53	...	...	21 1 53	Line destroyed by Turks near Soukhoum-Kaleh.
June	30 0 0	...	...	30 0 0	
July	31 0 0	...	...	31 0 0	
August	22 15 18	...	...	22 15 18	
September ...	2 13 44	...	...	2 13 44	
October ...	2 15 57	...	...	2 15 57	
November ...	5 10 43	...	...	5 10 43	
December ...	5 9 54	2 3 0	0 4 0	7 16 54	
1878.					
January ...	2 17 8	1 2 55	...	3 20 3	
February ...	0 22 46	...	...	0 22 46	
March	1 21 3	...	...	1 21 3	
	126 9 35	3 5 55	0 4 0	129 19 30	
	Comparison with Preceding 2 Years.				
1876-77	20 4 27	0 20 0	...	21 0 27	
1875-76	24 3 10	2 11 15	...	26 14 25	

Total Interruptions on Turkish Lines, 1877-78—Pera-Fáo Section.

	D. H. M.
1877.	
April	0 10 14
May	0 13 39
June	...
July	1 21 26
August	0 8 25
September	1 4 5
October	...
November	...
December...	4 5 31
1878.	
January	2 1 27
February...	1 5 18
March	1 23 46
Total...	13 21 51

Comparison with Previous 2 Years.

	D. H. M.
1876-77	30 7 0
1875-76	25 9 58

TEHRAN-BUSHAHR DIVISION.

Return showing the accuracy with which the traffic was conveyed over the lines between Tehran and Bushahr, and the percentage of errors of a serious and of a trivial nature for the year 1877-78, compared with the previous year

Month.	1876-77.					Remarks.
	Number of Words Transmitted.	Percentage of				
		Errors of a Serious Nature.	Errors of a Trivial Nature.	Words Mutilated in Transmission.	Words Correctly Transmitted.	
1876. April ...	38,563	0·020	0·165	0·185	99 815	
May	35,146	0·020	0·180	0·200	99·800	
June	28,313	0·095	0·280	0·375	99·625	
July	28,281	0·035	0·145	0·180	99·820	
August ...	37,167	0·020	0·150	0·170	99·830	
September	35,058	0·025	0·200	0·225	99·775	
October ...	39,756	0·075	0·310	0·385	99·615	
November	38,951	0·065	0·305	0·370	99·630	
December	27,549	0·025	0·205	0·230	99·770	
1877. January ...	44,488	0·050	0·230	0·280	99·720	
February ...	44,771	0·030	0·355	0·385	99·615	
March ...	46,396	0·070	0·285	0·355	99·645	
Percentage for the year	...	0·045	0·315	0·360	99·640	

Month.	1877-78.					Remarks.
	Number of Words Transmitted.	Percentage of				
		Errors of a Serious Nature.	Errors of a Trivial Nature.	Words Mutilated in Transmission.	Words Correctly Transmitted	
1877. April... ...	41,000	0·050	0·340	0·390	99·610	
May	17,262	0·045	0·240	0·285	99·715	
June	...	...	...	...	...	Total interruption of Company's lines.
July	...	...	...	...	...	
August ...	7,149	0·025	0·045	0·070	99·930	
September	22,495	0·015	0·040	0·055	99·945	
October ...	30,897	0·005	0·040	0·045	99·955	
November	24,699	0·050	0·180	0·230	99·770	Bad working. Messages retransmitted.
December	26,328	0·010	0·030	0·040	99·960	
1878. January ...	24,054	0·025	0·080	0·105	99·895	
February ...	24,060	0·020	0·115	0·135	99·865	
March ...	23,545	0·010	0·080	0·090	99·910	
Percentage for the year	...	0·025	0·120	0·145	99·855	

Showing the mean rate of transmission of messages between United Kingdom and India, *via* the Turkey and Tehran routes, during the year 1877-78, as compared with the previous year.

Month.	1877-78.			
	United Kingdom to India (Kurrachee).		India (Presidency Towns) to United Kingdom.	
	Via Turkey.	*Via* Tehran.	*Via* Turkey.	*Via* Tehran.
	D. H. M.	D. M. M.	D. H. M.	D. H. M.
April	1 2 53	0 0 55	...	0 1 15
May	1 8 18	0 0 46	...	0 1 23
June	0 14 53	0 9 26	...	...
July	0 13 2	0 14 5	...	...
August...	0 19 3	0 0 43	...	0 1 55
September	0 12 56	0 0 42	...	0 1 18
October...	0 21 33	0 0 38	...	0 1 33
November	4 23 24	0 1 28	...	0 1 26
December	1 23 16	0 0 37	...	0 1 31
January	1 16 18	0 0 39	...	0 1 16
February	4 8 17	0 0 45	...	0 1 18
March	1 13 58	0 0 37	...	0 1 9
Average through rate of transmission during the year	1 16 49	0 2 36	...	0 1 24

Month.	1876-77.			
	United Kingdom to India (Kurrachee).		India (Presidency Towns) to United Kingdom.	
	Via Turkey.	*Via* Tehran.	*Via* Turkey.	*Via* Tehran.
	D. H. M.	D. H. M.	D. H. M.	D. H. M.
April	0 18 11	0 0 47	...	0 1 22
May	0 15 49	0 0 41	0 15 50	0 1 15
June	0 13 7	0 0 50	0 12 30	0 1 15
July	0 20 20	0 0 56	...	0 1 23
August...	0 18 36	0 0 49	...	0 1 24
September	0 17 48	0 0 51	0 12 30	0 1 24
October	0 21 24	0 0 38	0 12 0	0 1 7
November	2 6 20	0 0 53	...	0 1 37
December	1 1 50	0 0 6	0 15 40	0 1 37
January	1 6 32	0 0 44	...	0 2 15
February	1 2 38	0 0 57	...	0 1 40
March	1 14 53	0 0 50	...	0 1 35
Average through rate of transmission during the year	1 1 7	0 0 55	0 13 42	0 1 29

Showing the comparative speed of transmission of messages from the United Kingdom to Calcutta by the Indo-European line, *via* Tehran, and the Eastern Telegraph Company's line, *via* Suez, as recorded in the *Government Telegraph Gazette*, during the year ending 31st March, 1878, and compared with the same information for the preceding year.

Month.	1877-78. Indo-European Route, *via* Tehran. D. H. M.	Eastern Telegraph Company's Line, *via* Suez. D. H. M.	Remarks.
April	0 1 14	0 1 9	Henjam cable section under repairs from noon, 20th, to 11 a.m., 22nd.
May	0 1 55	0 1 33	Tehran direct line between Soukhoum-Kaleh and Kertch interrupted from 13th May to 23rd August. Communication partially maintained by the Crown lines.
June	0 7 48	0 1 25	
July	0 15 28	0 1 34	
August...	0 5 15	0 2 1	
September	0 1 21	0 1 20	
October	0 1 3	0 1 20	Fao-Bashahr cable failed from 22nd to 26th October.
November	0 1 32	0 1 22	Henjam cable section under repairs, Nov. 1st, 4 p.m., to 3rd, 3.30 p.m. Fao-Bushahr cable interrupted 2nd to 17th Nov. 20th Dec., from 10.30 a.m. to 2.8 p.m. Total interuption Persian Gulf section on cable and land line. Gwadur-Jask section under repairs.
December	0 1 5	0 1 27	
January	0 1 3	0 1 13	
February	0 0 59	0 1 19	
March	0 1 1	0 1 21	
Monthly average during the year	0 3 19	0 1 25	

Month.	1876-77. Indo-European Route, *via* Tehran. D. H. M.	Eastern Telegraph Company's Line, *via* Suez. D. H. M.	Remarks.
April	0 1 10	0 1 18	
May	0 1 15	0 1 18	
June	0 1 13	0 1 11	
July	0 3 58	0 2 56	Kurrachee-Gwadur cable interrupted.
August...	0 1 51	0 2 0	Mekran coast land line interrupted 5 days.
September	0 1 28	0 3 4	Do. do. 2 days.
October...	0 1 7	0 1 59	
November	0 1 11	0 1 3	
December	0 1 21	0 1 43	Indiarubber cable interrupted for repairs. Indo-European Company's line interrupted 100 hours.
January	0 2 5	0 1 34	
February	0 1 20	0 1 25	
March	0 1 3	0 1 3	
Monthly average during the year	0 1 35	0 1 43	

The PRESIDENT: I myself have had, and still have, a good deal to do with these lines. I have not read this paper before, and I am not going to criticise it in any way; but I may say a few words to explain, what is no doubt well understood by many, but not by all, present, viz., why there is such a thing as a Persian line at all. When it was first proposed to establish telegraphic communication between India and England, the Red Sea was chosen as the route, and under the joint guarantee of the Treasury and the India Office a cable to India was laid by contract. In those days submarine telegraphy was not understood as it is now, and the Red Sea line proved a failure. That failure weakened for some time all confidence in submarine telegraphy, and the Indian Government and the Treasury decided that it would be unwise to renew the attempt by the Red Sea. The matter was postponed for a year or two; but after the Indian Mutiny the necessity for a telegraph from this country to India was more apparent than ever, and the Government determined to see what could be done by another route. One of the ablest officers in the service, Colonel Patrick Stewart, was consulted, and in 1862 he was deputed to visit and examine the Persian Gulf, to report whether a cable might run a better chance of lasting on such a bottom as that than among the coral rocks of the Red Sea. Colonel Stewart was allowed to take an assistant, and I was nominated to that post, and accompanied my chief in his first journey up the Gulf and to Teheran. In the meantime, endeavours were made to come to an understanding with the Turkish Government as to the best means of connecting England with the head of the Persian Gulf. It was assumed that the existing lines between London and Constantinople would suffice to carry the Indo-European messages, special wires being eventually set apart for the traffic. As for the onward link between Constantinople and the sea, the Turks had made the line as far as Bagdad, with the assistance of English officers and men, principally of the Royal Artillery. The Sultan's Government pledged itself to continue the wire from Bagdad to Fâo, provided our Government would lay a cable and meet them at the mouth of the Shat el Arab. As regards the Persian Gulf, it was found that the depth of water was suitable, the bottom was good, and it

was certain a cable could be laid; but the principal obstacle to the establishment of through communication by Turkey was met with between the Persian Gulf and Bagdad—a distance of 500 miles of desert country overrun by Arabs. The Turks used to work in the daytime, but what they then constructed was frequently cleared away at night. It seemed hopeless to place reliance on this section, but between Bagdad and Constantinople the country was comparatively settled. The best way to overcome the Arab difficulty was to avoid placing entire dependence on the Bagdad-Bussorah link, and to carry an alternative wire northwards to Teheran, rejoining the cable at Bushire. At last Arab opposition was to some extent removed, and both lines from Bagdad to the sea were completed in 1864; the working was never, however, entirely satisfactory. The irregularities were no doubt due—first, to the absence of special arrangements between London and Constantinople; and, secondly, to the supineness of the Turkish officials. At that time Dr. Siemens, and other gentlemen interested in telegraph questions, determined to attempt the establishment of a line which should avoid Turkey altogether, and the working of which should be from end to end in the hands of one instead of several small administrations. By the Turkish route was Belgium or France, Germany, Switzerland, possibly Italy—each state controlling but short lengths of line, and getting very little of the charges, and having, consequently, but small interest in the working of the traffic. The great object we had to try for was the establishment of one administration instead of many, and there was a fair prospect of getting a concession from Germany and Russia—the only two countries traversed by the Northern Line to Persia. I need not go into the details of the matter. The Indo-European Company's Telegraph was projected in 1867, and about the same time our rivals, the Eastern Company, convinced that we should not succeed in maintaining so extended a land line, resolved to try the Red Sea again. The result proved that both lines could be kept going. The Red Sea line was opened for work about the same time as that of the Indo-European Company, that is to say, in 1870. As to the former, the most difficult section is that between Teheran and

Bushire—a distance of about 800 miles. The Persian authorities rendered us but little help, and many of the influential classes, such as the priests, viewed our efforts with suspicion and even hostility. At last, however, by hard labour and perseverance, the line was finished, and may rank with the best land lines in the world. Complete interruptions are now rare, though they used at first to be frequent. The speed of working is really remarkable, and I may observe that nobody can form a just notion of what the distance traversed really is by a mere glance at this map.

A VISITOR: I don't know whether I am right in my note of the distances from the reading of the paper, but I make it a total of 1,248 miles. I understand you to say it is only 800.

The PRESIDENT: It is as nearly as possible 800 miles from Teheran to Bushire; and ever since 1870 that line has been working well, and we may, I think, claim credit for having brought it to a remarkable degree of efficiency. I may mention that I was struck a day or two ago, when looking over a list of complaints of delay in the time, to find that the worst case was one that a message was 4 h. 22 m. between Calcutta and London, which is not, I submit, a very discreditable example. I remember saying in this room, in 1864, that I hoped we should see a time when a message would be despatched to India and the answer received on the same day; and I believe I am correct in saying that I was considered over-sanguine. Now, on arriving at my office at 11 o'clock in the morning I often find messages which left India at 2 or 3 p.m. in the afternoon awaiting me. I have thus explained why the section which Mr. Preece's paper has described is especially interesting. It is a very important link in the chain of communication with India, and is a good instance of how a telegraph can be made and maintained in one of the most difficult countries the world can show.

Mr. LATIMER CLARK: It should be borne in mind that the half-past 2 and 11 are in the same day.

The PRESIDENT: The telegram arrives 3½ hours before it starts.

Mr. C. F. VARLEY: You have made some remarks, Sir, as to the failure of the original Red Sea cable. I happened to be one of the eight persons appointed by the Privy Council and the

Atlantic Telegraph Company to inquire into the question as to the possibility of laying safely deep sea cables, and I am glad to see that another member of that commission—Mr. Latimer Clark—is present. The real cause of the failure of the first Red Sea cable was not the coral rocks. The cable was sheathed with polished iron, and, that it should not rust, it was kept as dry as possible. Mr. Gisborne and Mr. Newall thought they achieved a great feat in covering a section of about 105 miles with 106 miles of wire strained. Now a cable so strained over a hard bottom would, of course, chafe through; but it was not that that caused the first failure. The cable was sent out dry—there was nothing to keep it cool—the heat was excessive, and the cable was bad before it was put down, as was the case with the first Atlantic cable. That was the reason why this cable failed. The fact that the Eastern Telegraph Company have cables still at work over the same route as that over which this particular cable failed, is a proof that it was not the coral rocks that cut it, but that something else than that did the mischief.

I was much surprised at hearing from this paper that such a thing as hammering wedges into porcelain insulators was attempted. About the year 1859-60 there was a stringent order issued by the Board of the Electric and International Telegraph Company, at my request, making it penal to hammer even the bottoms of the poles, because it cracked the insulators; and I am astonished that at this day the idea of hammering wires into iron caps should be adopted. In the older days of telegraphs, when Sir Wm. Fothergill Cooke erected them, the plan was to have stretchers at every half mile, the wires being suspended by intermediate poles being run through holes in the insulator. When Mr. Edwin Clark became the Engineer of the Company (1849-50) he swept these stretchers away, and had the wire bound fast to every insulator, and this system is the one at present adopted by the Postmaster-General.

Now, it is a singular thing that so much has been said in the paper about cobwebs being detrimental to insulation, and it would have been interesting to this Society if Mr. Preece, whilst he tells us what a quantity of cobwebs hung from the insulators, had taken a bundle of them and measured the resistance. I have done that myself time after time, but I have never been able to get a good

bundle of cobwebs well covered with dew, because, when cobwebs are new the water collects in globules, but when they are covered with smoke and dirt, in a warm atmosphere, then they may hold water in them, but not till then are cobwebs injurious to insulation.

I was pleased to hear that the resistance of the last insulators sent out for this line did not drop below one megohm per mile. Now, one megohm per mile is the standard I fixed for the Electric Telegraph Company, below which the insulation should not be allowed to go. In the paper it is stated that with the No. 5 gauge wire the insulation is often as high as 340 megohms per mile in damp weather. With a wire so well insulated in bad weather there should be no difficulty in working direct through it without translators, and I therefore come to the conclusion that there are some errors in the statements about the insulation. More precise details are very desirable.

As to wires getting into contact from natural causes, or being wilfully put into contact, it seems that the method adopted by the Electric Telegraph Company for preventing that has not been adopted in that part of the world. I have tried experiments entirely with regard to contact on the lines between Carlisle and Manchester, and Carlisle and Liverpool, and I found that generally when the wires swung against one another they remained in contact. That led me to stretch the wire, and elongate it $1\frac{1}{2}$ per cent., to take out the kinks, so that it might become "killed," while it was upon the ground before it was put upon the insulators. If the wire is "killed" first, all the crooks are taken out, and I defy anybody to twist those wires together (without making fresh crooks) so that they shall remain in contact; they untwist themselves; and I would only hint to engineers who may be engaged in erecting lines that it is a good plan to stretch the wire, elongating it 2 or 3 per cent. before putting it upon the poles.

Mr. Latimer Clark: The cast-iron sockets in this instance seem to have cracked in a somewhat mysterious manner. I would, therefore, inquire what cement was used in fastening the wrought-iron poles into the cast-iron sockets? I believe it to be sulphur, and it is possible that the expansion of the sulphur in combining with the iron when wet may be the cause. It has, as we all know

caused the breakage of insulators, and it may have caused the cracking of the poles.

Mr. W. MAYES: Sulphur and oxide of iron are used in about equal proportions as a cement.

Mr. VON TREUENFELD: I do not think that sulphur cement can have been the cause of the iron cracking. I think the iron must have been bad. Thousands of poles which I have seen fastened with sulphur cement showed no signs of cracking.

Mr. VARLEY: In Germany and Holland, and on the Electric Company's lines, this cracking takes place in the same way. It is the case on all lines that, where iron caps are used, and the insulators cemented with plain sulphur, they have invariably cracked in a short space of time by the combined action of iron, sulphur, and moisture. Dr. Siemens has introduced a mixture of hematite iron ore and sulphur, and that seems not to crack the insulator cups.

Mr. VON TREUENFELD: There is a contradiction between the statement of thePresident and that of Mr. Varley, who has suggested that hammering in wedges for securing the wire will destroy insulation. I do not agree with that, because I have never found it so. The President has stated that the Indo-European line is one of the most efficient as regards working capacity, and on this line a large number of the stretching insulators are wedged, which is a proof that the wedging of the wire does not destroy porcelain insulators. The Transandine line between Buenos Ayres and Valparaiso may be taken as another example of this. That line has such stretching insulators exclusively, and for 10 years it has shown complete success.

The PRESIDENT: I was not talking about the high insulation of the line, but only the general good results. As a fact, Persia is an excessively dry country generally. I do not argue whether this is or is not the best form of insulator, but I might almost say that, for the greater part of the year, over part of the line no insulation at all is necessary.

[Illustrating on the board, the President went on to explain.]

We are obliged to have an iron hood outside. The wedges are put in there [—], and it is not done so much by hammering as by

pressing the wedges in. At the same time, I admit that hammering is likely to crack the porcelain, but this is done only on every eighth post. We have tried various other forms for straining insulators.

Sir CHARLES BRIGHT: Mr. J. Preece, in advocating the construction of an additional land loop-line, and a junction with the sea line at Bunder Abbas, expresses some fear that the Persian Gulf cable of 1864 is showing signs of incipient decay. I am sure that no apprehension of the sort need be felt; that cable is laid upon a remarkably soft and favourable bottom, especially in the Gulf itself, and in water of easy depth for repair. The cable itself is well constructed, the conductor and insulator large; the outer wires of good dimensions, galvanised and protected by an outer covering of yarn and compound; so that, in my opinion, it can (certainly in the Persian Gulf, the largest part) be repaired without difficulty, and any small local defects be taken out and new pieces substituted, for the next twenty years at least. Such is my assurance from my personal knowledge of the cable itself, and of every part of the route over which I laid it.

The PRESIDENT: I had made a note of that, but omitted to refer to it. Mr. J. Preece, I can say from my own knowledge, is wrong in taking a desponding view of the 1864 cable. He would not himself lay claim to any special knowledge of its condition. He is, of course aware, that repairing operations frequently take place, but his duties are not in connection with the cable section. Mr. Preece is stationed about 200 miles from the sea. He introduces his remarks about the cable parenthetically, but there is in reality no ground for fearing that the 1864 core has suffered from any cause. As a matter of fact, there are places where the outer covering has yielded, and is more or less decayed. The cable has from time to time been raised for repairs, owing to accidents of different kinds, and the outer wires have been found to be corroded. The cable has had rather exceptional usage. At one place, it was shifted from Mussendom to Henjam. Several diversions have been made: once to get it out of the way of a mud volcano, and so on. It has had altogether an eventful life, but the core is as perfect as on the day it was laid. We have lately sent out

machinery to Kurrachee, and though we cannot turn out cable at great speed, we shall be able, I trust, to make it work as fast as occasion necessitates.

Mr. LATIMER CLARK proposed a vote of thanks, which was seconded by—

Mr. THEWLIS JOHNSON: I have great pleasure in seconding that proposition. I rise to supplement the remarks of Mr. Varley with reference to the wire supplied in 1863. It was not killed wire. The size was No. 5 Birmingham Wire Gauge. It was drawn from rods weighing about twenty pounds, and then welded into coils. The welds were lapped round with a piece of flat wire, and in the process of galvanising were soldered, so that if from any cause the weld broke, continuity would be maintained through the spiral. My remarks apply to the first wire supplied; the remainder, no doubt, was killed wire.

The following candidates were then balloted for, and declared to be duly elected:—

As Member:

James Grieve Lorrain.

As Associates:

C. J. Cole,

Leslie B. Miller.

The Eightieth Ordinary General Meeting of the Society was held on Wednesday evening, November 26th, at the Institution of Civil Engineers, 25, Great George Street, Westminster—the PRESIDENT, Lieutenant-Colonel BATEMAN-CHAMPAIN, in the Chair.

After the transaction of the preliminary business of the Meeting,

The SECRETARY read a paper

ON THE USE OF CONDENSERS,

By FELIX GARAY (*Foreign Member*),

Prefatory to which the Secretary said the author, in the first instance, sent over a very short note, which appeared in the current number of the Journal. Subsequently M. Garay sent a communication in continuation of the subject, and it was thought desirable that the whole thing should be read together.

ABSTRACT BY THE EDITOR.

M. Garay's second paper is a continuation of his former, given on page 357, Part 28, Vol. VIII., of the Journal, in which he endeavours to prove the analogy between a closed flood-gate, or lock, in a river, and a condenser inserted in a telegraph line in the manner condensers are employed in signalling through submarine cables, and in the second paper, among other things, he shows how to employ his reasoning to explain why a current divides itself inversely at the resistance of the path.

The great value of such scientific uses of the imagination consists in their leading to the predicting of unknown facts which, when tried experimentally, are found to exist in reality. In the present instance, however, it does not appear that the limits of electrical knowledge have been advanced by the speculations suggested in the paper.

At the close of the paper, the PRESIDENT inquired whether any gentleman present had any remarks to make upon it.

Professor HUGHES expressed his disapproval of such a paper containing so many untested theoretical considerations.

A second paper "On a New Method of Localising the Contact of Two Line Wires," by Fedele Cardarelli (*Foreign Member*), was then read by Mr. KEMPE, who illustrated on the board the formulæ referred to in the paper.

A NEW METHOD FOR LOCALISING A CONTACT OF TWO LINE WIRES.

By FEDELE CARDARELLI (*Foreign Member*).

Although there are several methods for determining the point of contact of two telegraph wires, still I trust it will not be useless to submit to the Society this method of mine, which is very simple, and the resulting formula does not depend on the diameter of the wires.

When two wires touch at a point and the contact offers no resistance, the locality of the fault may be easily determined. It is enough to measure, with the further extremities insulated, the resistance of the loop formed by the two wires. The distance of the fault from the station whence the experiment is made is given, as is well known, by the half of said resistance divided by the mean resistance of the wire per kilometre.

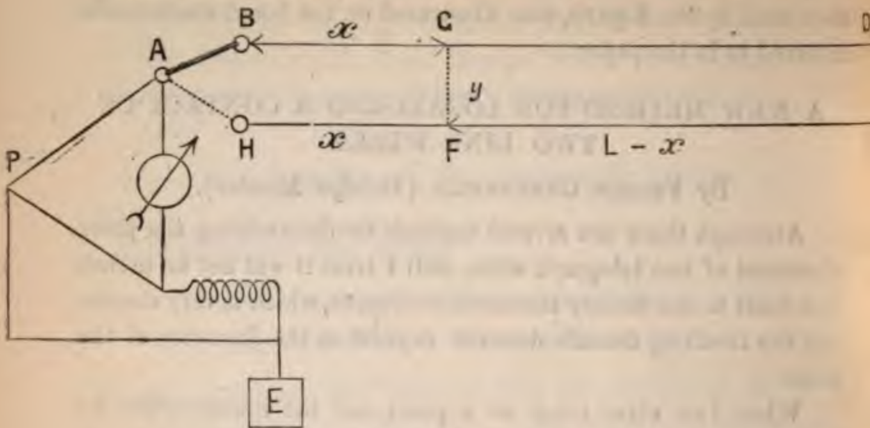
If the contact is not perfect, as very often happens, its resistance must be eliminated. To do this, as far as I know, there have hitherto been employed three methods:—(1) the bridge method; (2) Schwendler's; (3) that with the differential galvonometer, as given by Culley. Each of these methods has obvious advantages and disadvantages, but the following method devised by me seems to be both exceedingly simple and independent of the apparatus that may be used to measure the resistance.

Let B D, H N be two wires which are in contact at the points G F, one wire being insulated at its extremity, the end (N) of the other wire being put to earth. Connect with the apparatus you adopt to measure the resistance* the point B of the first wire, and measure the resistance, B G F N, which we may call R. If we call L the resistance of the line, then

$$R = L + y \text{..........................} (a)$$

* In the diagram the connections are made for a Wheatstone Bridge.

where L is the normal resistance of the line, and y the resistance which the contact offers.



This done, leave A B joined and connect the point A with the other wire at H, as represented by the dotted line A H, and again measure the resistance, which will naturally prove less than at first, since the new circuit is a derivation of the former one. This resistance will be composed of the two parallel derived circuits, B G F, H F, and the resistance, F N. Calling this r, we shall have

$$r = \frac{(x + y)\, x}{2\, x + y} + L - x. \ldots\ldots\ldots\ldots\ldots (\beta.)$$

Substituting in this equation the value of y of the former (α), simplifying and arranging, we have

$$x^2 + 2\,(r - L)\,x + R\,r - R\,L - L\,r + L^2 = 0,$$

whence

$$x = L - r \pm \sqrt{r^2 + R\,L - R\,r} = L\,r,$$

in which + must be taken, because from (β) we have

$$x = L - r + \frac{(x + y)\, x}{2\, x + y};$$

and as the quantity $\frac{(x + y)\, x}{2\, x + y}$ is always positive to $L - r$ we must add the root.

The formula to determine the distance of a contact between two wires may therefore be written as follows :—

$$x = L - r + \sqrt{(L - r)\,(R - r)} \ldots\ldots\ldots ()$$

Example.—Let 280 be the normal resistance of each of the wo wires, which, for simplicity, are supposed to be of equal iameters; 320 the resistance when only one, and 248 when both he wires are united to the point A of the diagram, as mentioned bove. We shall have

$$L = 280,$$
$$R = 320,\ L - r = 32,$$
$$r = 248,\ R - r = 72.$$

pplying the preceding formula we find

$$x = 32 + \sqrt{32 \times 72} = 32 + 48 = 80.$$

By this method also we can see at once whether the contact is erfect or offers resistance; for if $R = L$ there is no resistance in he point of the contact, whereas if $R > L$ the contact is not erfect.

This formula (γ) for localising a contact of two wires offers the dvantage of being independent of the diameters of the wires, for f the resistance of the piece of wire B C (see the diagram) is xpressed as a function of that of the other wire, we have the two ollowing equations:—

$$R = mx + y + L - x,$$
$$r = \frac{(mx + y)\,x}{mx + y + x} + L - x.$$

When in this last equation we substitute the value of y from the former, the quantity mx is eliminated, and we find

$$r = \frac{Rx - Lx + x^2}{R - L + 2x} + L - x,$$

whence we obtain the same equation as above—

$$x = L - r + \sqrt{(L - r)(R - r)},$$

where x is the resistance of the piece of wire from the station where the experiment takes place to the fault, referred always to the wire which has its extremity to earth.

In practice this method has given me satisfactory results.

FEDELE CARDARELLI,
Telegraph Engineer in the Italian Administration,
Florence (Italy).

The PRESIDENT said: This is a paper which it is perhaps somewhat difficult to discuss, seeing it is so full of mathematical formulæ, but possibly some gentleman will like to make some remarks upon it now.

Mr. KEMPE: It is similar to the well-known method of determining the position of a fault by two tests—one by carrying the end of the wire to earth, and the other end being insulated.

The SECRETARY said that Professor Ayrton, who was unavoidably absent, had mentioned to him that he considered Signor Cardarelli had sent an interesting communication on a new method for localising a contact. The method had, Mr. Ayrton thought, quite possibly the advantage of novelty, but he did not see that it was superior to the existing methods for ascertaining the position of such a fault. The objections to the majority of these systems, including that now brought before them, was that two successive tests were made to obtain two resistances, each of which included the resistance of the contact itself. If therefore, as would be very probable, this resistance altered during the testing, then calculation failed to give the required position of the contact.

The first method published for overcoming this difficulty, and the only one that can be employed when there are only two wires, was, as far as Professor Ayrton was aware, that given by himself in the *Engineer* several years ago, and subsequently reprinted elsewhere. The device, as might be expected, consisted in throwing the unknown and possibly varying resistance of the contact into the battery branch of the Wheatstone's bridge or differential galvanometer, exactly as was done in the ordinary loop test for earth.

The PRESIDENT: It only remains for me to announce that the next meeting will be held on December 10th. That will be the Annual Meeting of the Society, on which occasion the Annual Report of the Council will be read, and the officers of the Society for the ensuing year will be elected.

The Meeting then adjourned.

The Eighty-First Ordinary, and Eighth Annual, General Meeting of the Society was held on Wednesday evening, December 10th, 1879, at the Institution of Civil Engineers, 25, Great George Street, Westminster, the President, Lt.-Col. BATEMAN CHAMPAIN, R.E., in the chair.

The PRESIDENT announced that the ballot for Members of the Council and Officers of the Society for the ensuing year would remain open until 8.30 p.m.

Upon the motion of Mr. S. E. PHILLIPS, seconded by Mr. R. K. GRAY, the following gentlemen were appointed Scrutineers for the Ballot, viz., Mr. E. A. COWPER and Mr. VON FISCHER TREUENFELD.

The SECRETARY then read the Annual Report of the Council, as follows:—

ANNUAL REPORT OF THE COUNCIL.

Presented at the Annual General Meeting, Dec. 10th, 1879.

The Council are glad to report that the number of new members who have joined the Society during the current year may be considered satisfactory.

The addition to our ranks, including the gentlemen to be balloted for this evening, are as follows:—

Foreign Members	7
Members	8
Associates	43
Students	1

The list will, therefore, at the end of the year, subject to the result of this evening's ballot, be as follows:—

Honorary Members	4
Foreign Members	157
Members	329
Associates	506
Students	13
Total	1,019

On the other hand, the Society has experienced the average loss, from various causes, of members of each class. Among them we have specially to deplore the death of Sir William Fothergill Cooke, honorary member, Professor Clerk Maxwell, and Mr. R. S. Brough.

Sir William Cooke's name is so closely identified with the practical introduction of the electric telegraph, that every member of this Society must feel deep regret at his loss. A most interesting memoir of our late member has been prepared by our Past President, Mr. Latimer Clark, the first portion of which is already published in Part 28 of the Society's Journal recently issued.

The members of the Society, as well as all those interested in the history of the electric telegraph, are deeply indebted to Mr. Latimer Clark for having devoted much valuable time and labour to the compilation of so complete and instructive a record of the struggles and difficulties which beset the path of the distinguished subject of the memoir, in his early attempts to introduce the electric telegraph.

Professor Clerk Maxwell's death is a severe loss to science. He held a very prominent position in the front rank of those eminent philosophers who are gradually withdrawing our own particular science from the region of imagination to that of fact and truth; and his electrical works will become classical. His early death will be deplored by all members of this Society.

By a memoir of the late Mr. Brough, contributed by our distinguished member Mr. Schwendler, and also published in the last part of the Society's Journal, those members who were not acquainted with that promising young electrician will learn how serious a loss the Society and the profession generally have sustained by his death.

One most important event during this year in connection with telegraphy, was the assembly of the International Conference in London, during the months of June and July. The history and object of these periodical meetings were dealt with in the nt's address last January. At the London Conference of legates from nearly every civilised State in the world met he working rules and regulations as agreed in 1875 at

St. Petersburg. These delegates, among the most distinguished of which were some of the foreign members of the Society, were very cordially welcomed on their arrival by the Postmaster-General, and from the 10th June until the conclusion of their labours on the 28th July, they carried on their deliberations under the Presidency of Mr. C. B. Patey, Associate, and Assistant-Secretary to the Post Office. The revised rules will, it may be assumed, come into force from the 1st of April, 1880. Among many modifications introduced at the London Conference, the one most deserving of notice is the adoption in Europe of the system of charging messages by the word, instead of by the minimum message of 20 words, as at present.

The single word standard, as is generally known, has for several years past been accepted by the Extra-European administrations.

The delegates were most hospitably received and entertained during their stay in England by the Post Office authorities, and the leading telegraph companies in the country.

The Society was also enabled to welcome these distinguished visitors at a *conversazione*, given in the galleries of the Science and Art Department at South Kensington. It was the original intention of the Council to have collected for exhibition on that occasion electrical and telegraphic apparatus from America and the Continent, but the uncertainty which existed as to the date of the delegates arriving in London rendered this impracticable, and the exhibition was therefore confined to apparatus contributed by English exhibitors. The result was, nevertheless, generally admitted to have been highly satisfactory, a most interesting collection of apparatus, including a very complete, and indeed historical, series of telegraph instruments, kindly contributed by the Postmaster General, having been brought together. The Society is much indebted, not only to the exhibitors, but to those gentlemen who formed the Conference Reception Committee and the Apparatus Committee upon the occasion, as they devoted much time and attention to the details of the necessary arrangements, and especially the Sub-Committee, consisting of Professor D. E. Hughes and Mr. A. Stroh, who for several days continuously

devoted the whole of their valuable time in assisting the Secretary in the arrangement of the exhibits.

The liberal contributions towards the Conference Reception Fund not only enabled the Society to hold the *conversazione* on a scale worthy of the occasion, but left a surplus which, by consent of the subscribers, has been appropriated to the binding of the library.

During the session some very interesting papers have been read, viz. :—

On the Working of Long Submarine Cables, by Willoughby Smith.

On Curbed Signals for Long Cables, by James Graves.

A New Determination of the Ratio of the Electro-Magnetic to the Electro-Static Unit of Electric Quantity, by Professors Ayrton and Perry.

Experimental Researches into Means of Preventing Induction upon Lateral Wires, by Professor D. E. Hughes.

South African Telegraphs, by James Sivewright.

On the Effects of Induction between Wire and Wire with reference to the Electric Light, by W. H. Preece.

Recent Improvements in Professor Bell's Telephone, Illustrated by Experiments, by Adam Scott.

Note on Earth Currents, by William Ellis.

Some Historical Notes on the Electric Light, by Lieut.-Col. Frank Bolton.

The Telegraph from Teheran to Bushire, by J. R. Preece.

Note on the Use of Condensers, by Felix Garay.

On a New Method of Localising a Contact of Two Line Wires, by F. Cardarelli.

Colonel Bolton's paper will be found, by those whose attention is occupied by the subject of the electric light, a most valuable record of what has been done in the past in that branch of the science.

In the last annual report it was announced that the Council had appointed a committee to inquire into the question of the several wire gauges in use, with the view to obtain the adoption of one uniform gauge.

The Society has not been alone in appreciating the importance of some steps being taken to attain that object. The Chambers of Commerce of Birmingham and Glasgow, and subsequently, the Associated Chambers of Commerce of the United Kingdom, have had the subject under their consideration, and Sir Joseph Whitworth has also been moving in the matter. The consequence has been that the Board of Trade have instructed the scientific officers of the department to report upon the whole question.

The Committee of the Society has gone very fully and exhaustively into the matter, and their report, which is just now completed, will be forthwith communicated to the Board of Trade and the Associated Chambers of Commerce, from both of whom full consideration has been promised to it.

The Conference which, at the instance of the Meteorological Society, was appointed at the end of last year to draw up a set of rules in respect to the use of lightning conductors, has held a great many meetings, and has had under its consideration a vast amount of evidence bearing upon the subject, and its members expect shortly to be in a position to report the result of their labours.

The printing of the Ronalds' Catalogue is being proceeded with as rapidly as is consistent with a careful correction of matter composed of almost every known language; nearly 400 pages have been set up in type, of which 360 pages (viz., up to the end of letter M) have been finally corrected and printed off.

The Council regret to see that the list of subscribers to the Catalogue is not so large as, in their opinion, it should be, considering that the work is not only a most interesting and valuable register of more than 12,000 electrical publications of all nations, but contains, of course, the list of the books and pamphlets in the library itself. As regards these, the Council are glad to state that, by aid of the surplus remaining out of the Conference Reception Fund, they have been enabled, with the assent of the subscribers, to put in hand the work of binding, which is being carefully carried on under the superintendence of Mr. Alfred J. Frost, Acting Librarian, and already upwards of 300 volumes have been returned by the binders. It is expected, therefore, that in the course of the ensuing session, the library—which is the most valuable collection

of the kind in the world—will be in every respect ready for the use of the members of the Society and other students of electrical science.

One of the conditions of the Trust under which the collection was bequeathed to the Society by Sir Francis Ronalds was, that application should, within a certain time, be made for the Incorporation of the Society by Royal Charter, and accordingly, steps are being taken with a view to obtain this most desirable end.

Although the matter has not been formally brought before the members of the Society, many of them may have seen the announcement of the munificent offer of our Past President, Dr. C. W. Siemens, to contribute the sum of £10,000 towards the erection of a central hall for the domicile and use of the several Societies existing in London for the advancement of the applied sciences. A conference of representatives of these Societies has been formed for the purpose of considering the best means of giving effect to Dr. Siemens' proposal, and one advantage of the incorporation of the Society will be, that it will thereby be placed in a position to co-operate with the other societies in raising the remainder of the funds necessary to carry the plan into operation. In the meantime, the Society is to be congratulated upon the continued enjoyment, through the liberality of the Institution of Civil Engineers, of the use of their lecture hall for our meetings.

The attention of the Council has lately been seriously directed to the advisability of giving a wider scope to the proceedings of the Society. A misunderstanding appears to exist among many persons that only those who are either directly or indirectly *professionally* concerned with telegraphy are qualified to belong to the Society; others, again, have formed the idea that its proceedings are solely directed to that branch of electricity which consists of its practical application to telegraphic purposes. Now, a reference to the list of our members, and a glance at our published proceedings, should dissipate both one and the other of these erroneous impressions, but in the opinion of the Council it is most desirable that the whole matter should be put in a clearer light, so that there may be no misunderstanding as to the avowed objects *of the* Society as expressed in the first paragraph of the published

Rules and Regulations, viz., the general advancement of *electrical* and telegraphic science. It may be therefore found necessary to make some slight addition to the title of the Society, to indicate more distinctly that it is established quite as much for *Electricians*, both professional and non-professional, as for Telegraphic Engineers.

To this question the Council are giving their serious consideration, and they hope at an early meeting to offer to the members a recommendation on the subject.

The practicability of fixing certain evenings for the discussion on papers on purely electrical subjects, is another matter forming part of this question on which the Council will shortly be able to make a communication to the members.

The Council are glad to be able to report that the financial position of the Society is satisfactory, considerable economy has been effected in connection with the printing of the *Journal*, and they have already been enabled to pay off one-third of the loan which was contracted at the commencement of the year for the purpose of discharging the heavy debt then owing to the printers. The amount of annual subscriptions in arrears is unfortunately still much larger than it should be, and, but for this circumstance, the whole of the loan above alluded to might have been paid off this year.

Col. CROSSMAN : Mr. President, I have to move that the Report of the Council just read be received and adopted, and that it be printed in the Journal of the Society. I am sorry there is not a larger meeting before me to receive this proposition, but I am sure that the lucid report we have received from the Secretary and from the Council must make us congratulate ourselves upon the position the Society is taking in the scientific world.

Mr. R. K. GRAY : I have much pleasure in seconding the proposition.

The motion was carried unanimously.

Sir CHARLES BRIGHT : Gentlemen, there is one thing which we are not likely to forget, and that is the gratitude we owe to the Institution of Civil Engineers for their kindness in granting us the

use of this magnificent Lecture Room. The Institution of Civil Engineers, the oldest Institution of the kind in the world, and the largest, has always been ready to assist Societies such as our own by giving them similar help. I am sure that every one here will join with me in according them a very hearty vote of thanks for the accommodation which we receive at their hands.

Prof. HUGHES: I beg to second that; and I am sure we all feel very much indebted to the Council and Members of the Institution, and we cannot but heartily give them our fullest thanks.

The resolution was then put from the chair and carried unanimously.

Mr. MARCH WEBB then read his paper—"On the Operations connected with the laying of the New Marseilles and Algiers Cable."

AN ACCOUNT OF THE OPERATIONS CONNECTED WITH THE LAYING OF THE NEW MARSEILLES-ALGIERS CABLE.

By E. MARCH WEBB,
Associate.

I must apologise for the roughness of the drawings I have brought, and for the hurried manner in which this paper has been prepared, but in self-defence I must tell you that I did not expect this honour, and that I quite understood the few notes I had time to collect were to serve merely as a rough outline for a more elaborate article, to be prepared by a member of this Society, who is much more likely to do the subject justice.

In June, 1871, the India Rubber, Gutta Percha, and Telegraph Works Company laid a cable for the French Government from Marseilles to Algiers. The late Monsieur Ailhaud, Inspector-General of French Telegraphs, represented the French Government during the laying. Sir S. Canning superintended the laying. He was assisted by Messrs. F. C. Webb, Bell, and Herbert Taylor.

Only two types of cable were used—a light shore end, single sheathing of 12, No. 5 galvanised iron wire, weighing 4½ tons per knot; and a deep sea, sheathed with 16, No. 13 homogeneous wire, weighing about 1½ tons per knot.

The conductor was of 7, No. 22 copper wire stranded. The total weight of core was 273 lbs.

The *Caton*, French corvette, accompanied the *International*, and on approaching the African coast, piloted her into Algiers Bay.

The cable has worked very satisfactorily, and though the duplex system has been employed the traffic has so greatly increased, often amounting to 800 messages per diem, that the French Government was obliged to consider the advisability of laying a new cable. Tenders were invited, and the India Rubber, Gutta Percha, and Telegraph Works Company secured the contract.

The specification of the new cable is too long to be here repeated at length, but the following clauses in it may prove interesting. The specification was drawn up by the French Government Engineers.

The electrical conditions are: That the conductor resistance must not exceed 12 ohms per knot at 75° Fahr.; that the electrostatic capacity per knot must not exceed ·4 of a microfarad; that the insulation resistance per knot, reduced to 75° Fahr., after 1 minute charge, must not be less than 200 megohms; the conductor to be composed of 7 copper wires stranded, and to weigh 106 lbs. per knot; that the insulating material is to be 3 coatings of gutta percha, weighing 139 lbs. per knot, the total weight of core being 245 lbs. per knot.

The mechanical conditions were: That the deep sea type of cable be composed of 15 homogeneous wires, No. 13 gauge, and the cable have a minimum breaking strain of 5½ tons, and the weight per knot being 1·58 tons; that the intermediate type of cable be composed of 10 galvanised iron wires, No. 6 gauge, and the cable have a minimum breaking strain of 6 tons, and its weight per knot being 3·16 tons; that the heavier type for shore end be composed of 10 galvanised iron wires, No. 0 gauge, and the cable have a minimum breaking strain of 15 tons, and its weight per knot being 10 tons.

These weights are wet in air.

The shore end and intermediate types were served and coated

with compound in the usual manner. The deep type was served with two layers of tape, and coated with compound.

The galvanising was tested by 4 successive immersions of the wire, of 1 minute each, in a solution composed of 1 part sulphate of copper to 5 parts water.

On the table there are sample cases, shewing the different types.

The s.s. *Dacia* owned by the Company, left the Thames on September 5th, with the cable destined for the new line.

The *Dacia* is a screw steamer of 1,856 gross tonnage. She is 283 feet long, 34·7 beam, and 17·9 depth of hold. Her engines are compound, of 170 nominal horse-power. She has 4 cable tanks, in which can be carried 1,500 tons of cable, besides 5 smaller tanks, in which are coiled different lengths and sizes of buoy and grappling rope. She has an exceptionally fine between-decks, which is a matter of great importance for carrying on such work as joints, splices, etc., in bad weather.

The dynamometers for the picking-up and paying-out gear are of the usual vertical description, and have 4-sided scales attached, showing the various strains calculated to such different weights as may be placed on the dynamometer carriage. To the sliding wheel is affixed a pointer, which, as the wheel rises and falls with the varying strains, indicates the latter on the scale.

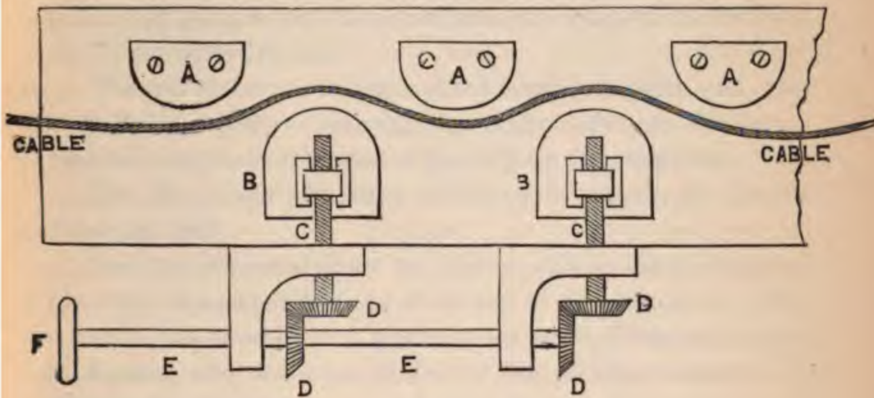
To the paying-out drums are attached a pair of small horizontal engines, fed from the main boilers. These engines are found very useful in starting the drum and picking-up on it. The drum itself is held by a very powerful friction strap-break. The engines are about 20 horse-power.

A little forward of the paying-out drum there is a break or holding-back gear, consisting of two rows of semicircular iron plates placed upon a heavy baulk of timber, thus:

One row of plates is movable, and when approached to the other can, if required, completely hold the cable.

The plates are moved as seen in the figure.

This break has been found to work very well, owing to its simplicity and the quickness with which break-power can be applied. It was first fitted to the *Dacia* during the latter part



HOLDING-BACK GEAR.

A, A, A, are iron segments firmly screwed on to a baulk of timber.

B, B are iron segments worked by screws C, C, which are moved by bevelled wheels D, D, attached to the shaft E.

The pressure, and consequent friction, which can be applied to the cable by increasing or diminishing the distance between the movable segments, B, B, and fixed ones, A A, A can be regulated, by turning the wheel F, to give any holding-back power required, or to allow the cable to pass perfectly freely.

of the first West India Expedition, and was afterwards considerably improved by Mr. F. C. Webb.

The engines of the picking-up gear are double cylinder of about 50 horse-power. I have seen them break a double sheathed shore end.

When the *Dacia's* main engines were compounded a reducing valve was placed in the steam pipe between the boilers and the picking-up gear, so that the pressure on the cylinders of the latter should not exceed 30 lbs.

The rest of the machinery is of the usual description, and offers no sufficient novelty to warrant any further account. There is a plan and longitudinal section of the ship for your inspection.

The *Dacia* was first fitted out for cable work by Sir Charles Bright in 1869.

The ship proceeded direct to Algiers, and on the afternoon of the 16th September laid the shore end of the new cable. This operation was watched with great interest by the Governor-General of Algeria, who was accompanied by the principal Government officials of the Colony.

The operation of landing a shore end, as carried out by this Company's Engineers, is as follows: The ship being anchored so near the shore as is found convenient, a warp is passed over the stern, is sent ashore, and led through two spider wheels placed so far apart as possible and firmly anchored by mushrooms. The warp is brought back on board and taken to the picking-up gear. The shore end is made fast to the warp, passing over the stern, and slowly paid out. As the cable is paid out, barrels, fitted (according to a plan devised by Mr. F. C. Webb) for the purpose, are attached to it at about 8 fathoms apart. While the cable is paid out the picking-up gear heaves on the warp passed forward, and thus drags the cable, supported by the barrels, ashore. Any deviation from a straight line due to tide or current can be straightened out by stopping the paying-out aft and heaving in forward.

On several occasions during the laying of the Peru and Chili cables we took advantage of the proximity of a railway by making fast the warp to a locomotive, instead of bringing back on board to

the picking-up gear. This was, as may be imagined, a very comfortable and speedy way of landing a shore end.

In the above manner of landing a shore end, no shore labour is required, a matter of great importance, and in fact only four ship's hands are necessary on shore for the purpose of keeping the lines clear.

Another advantage is, that instead of throwing the cable over in bights, as may happen when only boats are employed, the cable lies on the bottom in a perfectly straight line, and not with one turn over another, as sometimes may occur.

Again, and it is a very important point, that the engineer on board has the whole operation completely under his eyes, as he can control the paying out of the cable and the heaving in of the warp.

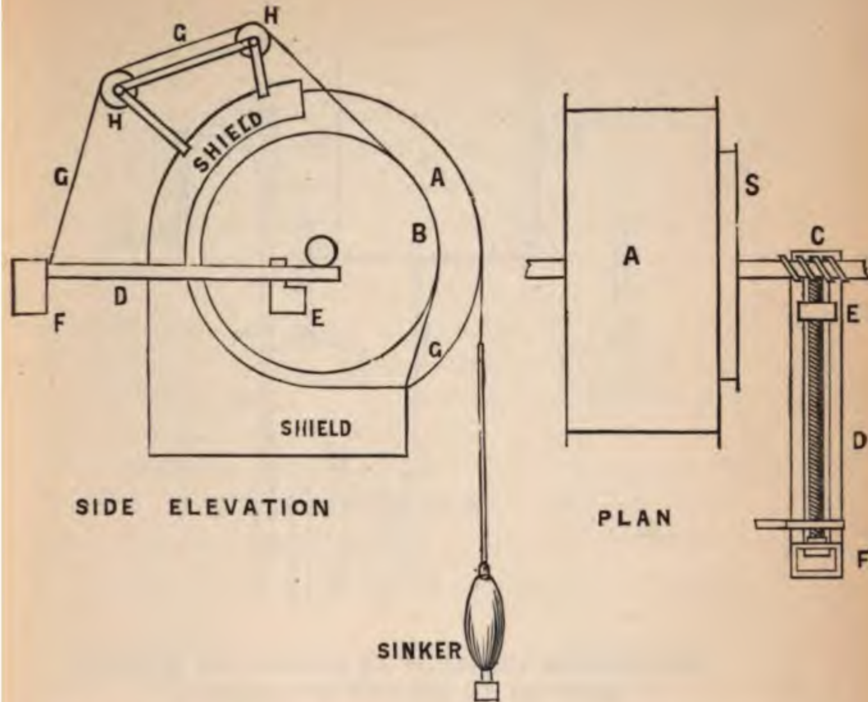
A great advantage in using barrels is that a shore end can be landed when boat work is out of the question through heavy surf.

I remember a case in point during the same Peru and Chili expedition, when landing a shore end on a very exposed beach. The boat sent ashore with the lines capsized, and the hands were only saved through wearing life belts. The boat was rescued from the breakers by some fishermen on shore, with the lines still in it. The crew managed to get on shore, some by clinging to the boat and some by swimming, but all much exhausted. We landed two shore ends that morning.

The *Dacia*, after successfully completing the landing of the shore end, ran, paying out cable, to the position fixed upon, and buoyed the intermediate end in about 600 fathoms. 6·94 n.m. of shore end, and 2·97 n.m. of intermediate were laid.

The *Dacia* then immediately commenced sounding towards Marseilles, along the proposed route of cable. The soundings were taken night and day. At night the operations were greatly facilitated by the use of the electric light, which brilliantly illuminated the after part of the ship. For this purpose the *Dacia* carries two engines of 5 horse-power each, working two Gramme machines. The Serrin lamp, slightly modified for work on board ship, was used alternately with a lamp called the "Silvertown Hand Regulator."

Sir William Thomson's sounding apparatus was employed, and



SELF-ADJUSTABLE BREAK ON THOMSON'S SOUNDING MACHINE.

A. Drum.

B. Flange wheel (screwed on drum), and round which the break strap passes.

C. Worm cut in end of drum spindle, working in worm wheel at end of lever D.

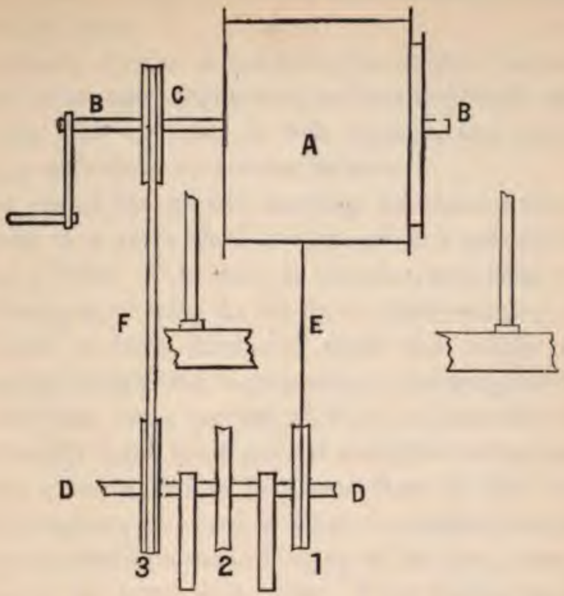
D. Fore and aft shaft, acting as a lever, threaded. Worked by worm wheel at C.

E. Counter poise, running on guides. The threaded shaft D runs through it, and as the drum revolves works it outwards in proportion to the amount of wire paid out.

F. Fixed weight at end of lever D. It regulates the speed at which sinker descends.

G. Break strap. One end fastened to shield, and after being passed round B, runs through the pulleys H, H, and is connected to lever at F.

H, H. Pulleys guiding the break strap from B on to lever.



POSITION OF PULLEYS ON SOUNDING MACHINE FOR FACILITATING PICKING UP OF WIRE.

A. Drum.

B. Drum spindle.

C. Pulley on drum spindle, connected by a band F to pulley No. 3 on lower shaft.

D. Lower shaft, to which are attached 3 pulleys, 1, 2, and 3. Pulley No. 1 carries the wire E when picking up on to drum. Pulley 2 is connected to after steam winch by a band. Pulley No. 3 is connected to pulley C on drum spindle by band F.

E. Wire, which, when picking up, is passed round pulley No. 1.

F. Band between pulleys 3 and C. When the steam winch is required for picking up, this band is taughtened by a block and tackle. The band from steam winch hangs loose until required. It is then thrown on pulley No. 2 and taughtened by block and tackle.

The strain when picking up is thus thrown on the lower shaft, and the wire coiled on the drum by means of the two pulleys 3 and C, and band F.

used successfully in some very rough weather, caused by a "Mistral," which is the north-west wind sometimes encountered in the Gulf of Lyons.

The ordinary manner of balancing the weight of wire paid out is to place the counter weights on a rod attached to one end of the break strap, but we used on this occasion, and very well it answered, a self-adjustable counter-balance.

On the end of the spindle carrying the drum a worm is cut, which works in a worm wheel on the end of a fore-and-aft shaft, acting as a lever. This shaft is threaded, and runs through a weight running in guides. As the drum spindle revolves, the fore-and-aft shaft or lever turns and works the weight outwards, increasing the break power in proportion to the weight of wire paid out, for the break strap, one end of which is secured to the main body, is brought round the drum and made fast to the lever.

At the extreme end of the lever there is fixed a weight, intended to regulate the speed at which the sucker descends.

In order to facilitate the picking up of the wire, three pulleys are placed in the lower shaft, thus. When picking up, the wire passes round the first pulley and on to the drum. The third pulley is connected by a band to a pulley on the drum spindle, and the second pulley is connected by another band to the after steam winch.

Thus the strain comes on the lower shaft when picking up, and the wire is coiled on the drum by means of the band connecting the third pulley to the pulley on the drum spindle. The wire used was steel, gauge No. 20, weighing about 15 lbs. per knot. Breaking strain, when new, a little more than 200 lbs.

The solution used for preserving the wire from rusting was composed of three ounces of caustic soda to one gallon of pure water.

I may here remark, that in order to expedite the work a spare sounding apparatus was afterwards placed on board the *Charente,* French telegraph steamer. The French officials were so pleased with it that we left the machine on board their ship.

Between the 17th and 23rd September, the time we occupied in running to Marseilles, the *Dacia* took 93 soundings, in addi-

tion to the 40 already existing near the route of the cable—making 143 soundings along our line.

An average day's work (24 hours) would consist of from 10 to 11 soundings at intervals of 7 to 10 miles, in depths varying from 1,400 to 1,500 fathoms. The greatest depth encountered was 1,536 fathoms. The bottom was fairly uniform in character, and consisted of clay and ooze.

Mr. W. H. Preece, who was on board during the laying of the cable, examined the soundings under his microscope, and found principally Globigerinæ and Orbulina, like the ooze of the Atlantic. I may add that Mr. Preece has kindly promised further information on this subject.

The average time occupied in sounding in depths of 1,400 to 1,500 fathoms was from 25 to 30 minutes.

On our arrival at Marseilles, we found the *Charente* French Government Telegraph steamer, commanded by Captain Cavallier, of the French Navy, awaiting our arrival.

The *Charente* is a screw corvette of about 1,000 tons. Her picking up and paying out machinery is of excellent quality and very well arranged, and was designed by the late Monsieur Ailhaud, and by Monsieur Wünschendorff, Inspecteur Ingenieur of French Telegraphs. She is a most handy vessel, and well adapted for cable work in moderate depths. The manner in which she is equipped reflects great credit on the French administration. Her crew is drafted from the French Navy.

The operation of landing the shore end at Marseilles was effected during the afternoon of September the 28th, in the same manner as at Algiers, but the time occupied was considerably longer, owing to the fact that the cable hut is about a quarter of a mile from the beach, on the bank of a small stream along the bed of which the cable had to be laid. The stream was about 3 feet deep, and running very strongly, owing to floods. The cable was carried up the stream in three boats towed from the bank, and we had rather a difficult job to get them under a low bridge at the mouth of the stream. 9·05 n.m. of shore end and 5·02 n.m. of intermediate were laid.

A very amusing incident occurred whilst landing the shore end.

A gentleman, much decorated, and evidently a great authority, was explaining the operations to an admiring circle of friends, and on being asked why the cable was supported by barrels, informed his hearers that it was for the purpose of keeping the cable dry.

The various sections were spliced up in one length before commencing to pay out, in order to avoid any splicing during the paying out.

At 4.54 a.m. on September 29th, the ship started paying out cable towards Algiers. At first the speed was 6 knots per hour; it was afterwards increased to 7, which rate was maintained till a few hours before reaching Algiers. The speed was slackened so as not to reach the buoy at intermediate end before daylight.

During the laying, the paying out machinery was illuminated by the Electric light. It is impossible to over-rate the value of this mode of lighting for work of such a nature. Every part of the machinery stood out clearly and distinctly as in daylight, and you may imagine what an advantage the being able to dispense with the usual fixed and hand oil-lamps was.

When paying out cable we employ Hearson's "Strophometer," as made by Elliott Brothers. This instrument was used by the Company on the Peru and Chili expedition, and has been found of great service.

It consists of a dial face, numbered, round which a pointer moves.

The instrument is connected to the paying out drum, and indicates the least fluctuation in speed.

By watching it carefully, those who have been accustomed to its use can tell when the depth changes to any material extent.

A detailed description of this instrument is to be found in the Transactions of Naval Architects for 1874.

I have brought the instrument used on this expedition. You will see it on the Table.

For the purpose of facilitating communication between ships engaged in cable work, the Company's Engineers have drawn up and had printed a code of signals. You will see on the table a few copies of the work.

This code is gathered from various sources, and has taken some

time to compile. The system adopted is a three flag hoist for each signal, which represents one or more words or a whole sentence. The flags used are those employed in the usual commercial code. The arrangement is such that not only the letters representing the flags, but also the words or sentences themselves are arranged in alphabetical order.

As this paper treats of cable work generally, I would call the attention of the meeting, that principally through the exertions of Her Majesty's Post Office the Board of Trade has issued a circular, dated 14th August of this year, in which appears a clause (Article No. 5) referring to cable ships. With the permission of the meeting I will read this article.

"A ship, whether a steamship or a sailing vessel, when employed either in laying or in picking up a Telegraph Cable, or which from any accident is not under command, shall at night carry, in the same position as the white light which steamships are required to carry, and, if a steamship, in place of that light, three red lights in globular lanterns, each not less than 10 inches in diameter, in a vertical line one over the other, not less than 3 feet apart; and shall by day carry, in a vertical line one over the other, not less than 3 feet apart, in front of but not lower than her foremast head, three black balls or shapes, each 2 feet in diameter.

"These shapes and lights are to be taken by approaching ships as signals that the ship using them is not under command, and cannot therefore get out of the way.

"The above ships, when not making any way through the water, shall not carry the side lights, but when making way shall carry them."

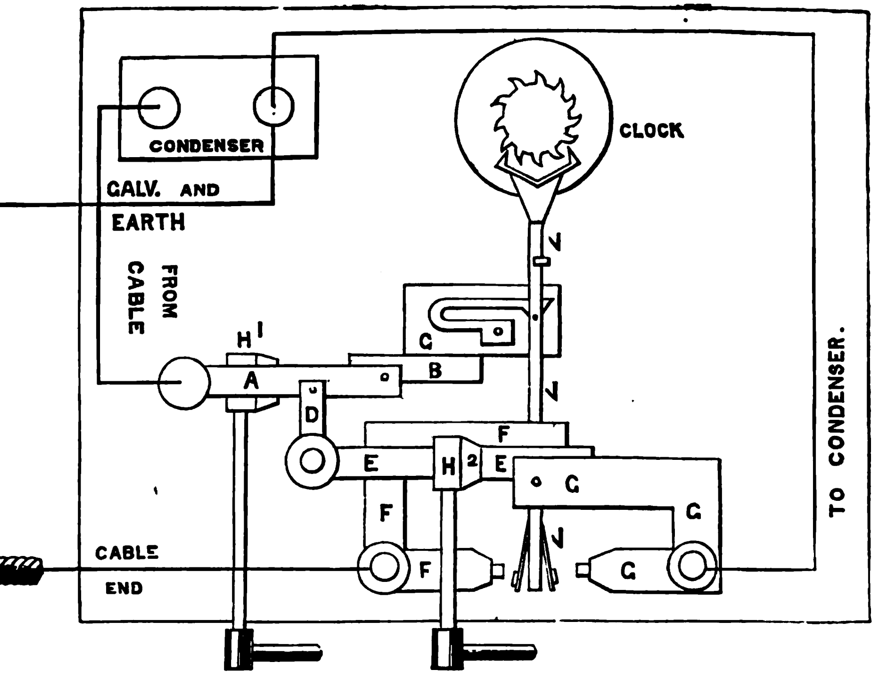
These regulations apply on and after the 1st September, 1880.

The continuity signals between the ship and the cable-hut at Marseilles were carried on by means of a clockwork discharger at the hut.

You will see one of the instruments on the table.

This clockwork is similar in many respects to the one used by Mr. Herbert Taylor, who was chief electrician during the laying of the 1871 Marseilles-Algiers cable, and I believe that Mr. Henley, during the laying of one of the Red Sea cables, used a pendulum to make the contacts.

I think also that Mr. F. C. Webb, during the repair of the Hague cables, used a clockwork apparatus, but I am uncertain as to its principle of working.



CLOCKWORK DISCHARGER.

A. Brass tongue. Its normal position while the clockwork is in operation is on contact plate B.

B and C. Brass contact plates. On C there is a spring. Contact is made between the lever L and the spring by means of a pin projecting from lower side of lever. There is also a spiral spring between the lever and plate C, to ensure good contact.

D. Contact plate. Free while clock is working.

E, E. Brass tongue. Its normal position is on contact at G.

F. Contact plate, to which cable end is attached.

G. Contact plate, to which the earth side of condenser is connected.

L. Lever worked by clock. Makes contact every fifth minute on anvil at F, returning, after making this contact, to G.

H1. Cam on end of hand lever. Used for moving tongue A from normal position on B to contact at D, thus switching out connection with lever L when signals are to be given by hand.

H2. Cam on end of hand lever. Used for giving signals by hand (when clockwork and lever are cut out), by pressing tongue E down on plate F, to which cable is attached.

When clock is at work the current passes from F to lever L, on to plate C, and thence, *via* B and A, to condenser. When hand signals are given, the current passes from F to E, D, and A, and thence to condenser. When E returns to normal position at G the condenser is discharged.

The apparatus consists of a clockwork arrangement moving a lever, which every fifth minute makes contact between the cable end and one pole of a condenser, the other pole of which is connected to earth through the galvanometer.

A current is applied on board through the marine galvanometer, and when the lever of the clockwork makes contact between the cable-end and the condenser, a signal is observed simultaneously by ship and shore. Between these fifth minute contacts the cable-end is insulated, the lever moving on to the opposite contact point, thus discharging the condenser.

The current is reversed after every 12 continuity signals, that is every hour, and should ship desire to communicate with shore the current is either reversed before the 12 signals have been completed, or the same current is continued beyond the 12 signals, instead of being reversed after the twelfth.

In order that shore may be able to call ship, two hand levers are attached, by means of one of which the clockwork is switched out, and with the other the signals may be given as rapidly as may be desired by hand.

The condenser power on shore is adjusted according to the length of cable in circuit and the deflection required on board ship.

The advantages of this system are: immunity from any trouble caused by a bad watch at hut and perfect regularity of signal.

In order to avoid any touching of the cable end, a closed commutator is used, to the centre pillar of which the cable is permanently attached. The cable at will can be transferred from the clockwork to either the instrument, testing apparatus, or earth.

The only stoppages made during the laying were on the occasions of changing tanks, always a critical operation in deep water.

The weather had been perfect, the cable had run out smoothly, the test room reports were satisfactory, and all on board expected a speedy conclusion of the work, when at 6.30 a.m., October 2nd, the buoy on Algiers intermediate was picked up.

I have brought a chart showing the lines of old and new cables.

But when heaving up the buoy moorings attached to cable, it was found that the latter had fouled some rocks, and while

manœuvring in order to free the cable, a defective link in the chain parted.

The defective link is on the table. There may be a dismal sort of satisfaction derived from seeing the object which retarded what otherwise would have been a speedy piece of work. After several attempts to grapple, it was found that the ship—a strong breeze then blowing—was, owing to her lightness, quite unmanageable, and it was decided to run into Algiers Harbour and ballast.

The *Charente* meanwhile had been grappling, but also unsuccessfully.

The bottom was found to be ooze between patches of rock and lines of reefs, and at the point where the cable had been buoyed there was a rapid descent seawards.

On entering the harbour of Algiers for the purpose of ballasting, the unwelcome news was communicated to the ships of the sudden breaking down of the old cable, and it was assumed that the cable had been broken during one of the dredges. This at first appeared impossible, as the line of the old cable as laid down in the charts had certainly not been crossed by either ships, but afterwards it was ascertained that the actual position of the old cable was more to the eastward than as laid down in the charts.

After ballasting, and transferring to the *Charente* some cable for the repair of the old Marseilles-Algiers line, the *Dacia* put down buoys in such positions as were deemed necessary. Further attempts to grapple the cable near the end were then made, but were unsuccessful owing to the rapid slope of the ground, and the rocky nature of the bottom.

The cable was then grappled nearer shore, the shorewards end buoyed, and the part seawards picked up until near the end originally buoyed when the cable parted, being firmly held by rocks.

The cable was again grappled nearer the seawards end, and the piece originally buoyed (that is the intermediate end) picked up.

With the cable we picked up the part of the mooring chain which had been carried away, and on the chain the defective link.

Altogether a little more than a third of a mile of intermediate ‹e was lost, this portion being firmly held by rocks.

The *Charente* meanwhile was engaged in grappling and in picking up the old cable, not so easy a task as would at first appear, owing to the foul ground in which the cable was constantly held, necessitating frequent breakage.

On this smaller chart you can see the various drags made by the *Dacia* and *Charente*.

On October the 11th the *Dacia* joined up the intermediate recovered to the shore end lately cut and buoyed, and splicing up a piece of fresh cable to replace that lost, ran out to buoy on the Marseilles end (I have forgotten to mention that when the chain parted we cut and buoyed the deep-sea cable), and completed the new cable.

The delay in finishing the work was due to the necessity of allowing the *Charente* to conclude her operations on the old cable, as the two lines ran so close together.

The old cable was found, wherever examined, to be in as good condition as when first laid, the galvanising being perfectly bright and sound throughout, although the cable had been submerged for eight years.

I have brought some pieces of the old cable with me in order to shew the preservative nature of the bottom.

For the instruments, plans, &c., I have brought to-night, I need hardly tell you that I am indebted to the India Rubber, Gutta Percha, and Telegraph Works Company.

I only wish more time had been at my disposal for availing myself of their kindness.

I may add that the rates on the old cable were twopence per word from Algiers to any part of France. Since the new cable has been laid the tax has been decreased to one penny per word, a very low rate considering the length of cable.

And now, gentlemen, I must thank you for the kind attention you have given me during the reading of this paper, which, as you are now able to judge, shews only too well how hurriedly it has been prepared. The *Dacia* has only been back three weeks, and I have had a great deal of work to attend to connected with this expedition, and the repairs of the Lizard-Bilbao cable.

Col. CROSSMAN: I had, sir, the pleasure and great privilege of being on board the *Dacia* when the operation of paying out the cable of which Mr. Webb has given us so interesting an account was being carried on. I do not profess to be an electrician. My duty as connected with submarine work is rather blowing off communication with people in the outer world than it is in connecting people with other people, as in the case of the operations referred to. I must say this, that I never felt greater pleasure than I did in being on board that ship. The Members of this Society have great reason to congratulate themselves upon having amongst their body men who can do their work as I saw them do it on board that ship.

Mr. FREDERICK WEBB: I have no particular remark to make, except that I think the paper one that does great credit to Mr. March Webb. It is some time since we have had any full description of the laying of a cable, and in our present state of knowledge as regards laying cables, it is rather a reproach to the Members of this Society that there has not been something of the sort before. We have many companies and firms that have laid cables, but this is the first systematic description that we have had of the laying of a cable. It was stated that it was written hurriedly, but I think it is fuller than any description given yet, particularly with the full longitudinal section of the *Dacia*, which was originally designed and fitted by Sir Charles Bright. I think this is the first time that a full longitudinal section of a telegraphic ship has been exhibited before either the Institution of Civil Engineers or this Society since I exhibited a longitudinal section of the s.s. *Monarch*, before the Institution of Civil Engineers, in February, 1858. I think the improvement in the sounding machines, as far as I have seen from the drawings, is very ingenious, and is likely to be used again. The idea of having the weight to screw itself along the break lever, so as to compensate for the weight of the wire as it goes out, is very ingenious, and, I think, should lead to further improvements in the same direction. With regard to the holding-back gear, I think that was first designed by Mr. Matthew Gray when laying some cables on the *West India* Panama Line. I simply improved it by putting the

screw arrangement, so as to make the two parts of more easy adjustment by hand. The simplicity of that plan, and the fact that it does not carry any inertia with it, or drag any machinery into motion, is of very great advantage, and when you see the drawings of the machinery in the *Great Eastern*, with some five wheels, each with a jockey-pulley on it, one is astonished to find that so many have been used. The machine shown has held the cable in the same depth of water as the machine used on the *Great Eastern.* I have seen myself, when paying out a cable in deep water and the ship has been pitching, the holding-back gear with only two wheels and break straps used, move after the break stopped and thus slack the turns on the drum, thus causing the cable to slip on the drum, which is the most dangerous thing that can occur. When you are paying out a cable and it begins to slip, it is likely to run away altogether, as it did in some of the early Mediterranean cables. I consider that the holding-back gear is a great improvement on the old one with wheels. Of course, there are many things with regard to the complete fitting of the ship which do not bear on the subject of this paper; but with regard to the clock arrangements Mr. March Webb has mentioned that I used on the Hague cables, it was a clock sending for five minutes a current, then insulating for five minutes, and then putting to earth for five minutes. Mr. Latimer Clark suggested it. The same thing was used on the Red Sea cable by Messrs. Siemens when acting as electricians for Messrs. Newell. In the Persian Gulf we used an arrangement—from which I believe the one described in the paper was partially taken—designed by Messrs. Latimer Clark and Co. and Mr. Laws, in which the condenser was first used; but I believe this was further improved by Mr. Herbert Taylor for the first Algiers cable, and has since been improved by Mr. Robert Gray, to whom great credit is due for the completeness of the work, including soundings, the adoption of the electric light, and, indeed, all the careful steps taken in the work of the Algiers cable, so ably described by Mr. March Webb.

Sir Charles Bright: The holding-back gear which Mr. March Webb has described as being on board the *Dacia*, and which Mr. F. Webb has just referred to, I had made in the

West Indian Cable Work, in 1871. I had fitted on board the *Dacia* the same system of V sheaves and jockey-pulleys for holding-back gear as was used for the Atlantic cable and some other cables, but I found a good deal of inconvenience in a warm climate from the compound sticking in the sheaves, and so I therefore devised this arrangement. The *Dacia*, which Mr. March Webb has referred to, was originally built by Laing, of Sunderland. I had her fitted up for the object of making her, as far as I could from my experience, a thoroughly complete cable-laying and repairing ship, and I think that the character which she has had since in every expedition, proves that she is a success. The tanks I had put in at Sunderland where the ship was strengthened in various parts. The machinery was made from my designs at Messrs. Easton and Anderson's, and I think I may say that the paying-out and picking-up gear are not excelled by any machinery of the same kind on any other vessel. She has shown her capability of doing good work on many occasions, and I remember hearing that in the repairs of the direct Atlantic cable immediately after the laying, when the machinery of another ship broke down, the end of the cable was passed over to the *Dacia*, and she brought up the cable easily. Mr. Webb's paper is so clearly written, and touches on so many points, and is withal so modest a description of a work excellently carried out, that I have very little to say in comment upon it. There seems to have been no hitch in the work, and everything seems to have been a success. I congratulate Mr. Robert Gray, and all the other gentlemen who took part in the operation, upon the successful way in which so important a work in deep water has been carried out, without any of those vicissitudes which those who go to sea to lay cables are unfortunately sometimes likely to meet with.

Mr. PHILLIPS: Is there not some risk of the barrels being carried down with the shore end in landing? They have to be removed, of course, and as they are removed there must arrive a point when the barrels are no longer able to support the bight. I should think it would be a dangerous thing to have them carried down while they hold fast to the cable. Possibly they have some method of taking them off simultaneously all along.

Mr. R. Gray: Perhaps I can answer that. We laid a cable once with barrels at Dieppe when the tide was running strongly, but by anchoring the ship firmly, and heaving strongly, we could draw the line always straight. It is a little off the line, but very little.

Mr. F. Webb: Sir Charles Bright has mentioned the holding-back gear was designed by him. I have always understood (of course I may be misinformed) that Mr. Matthew Gray designed it at the time of laying the West India cable, but I may have made a mistake. Perhaps it was when he was there with Mr. Matthew Gray. The screw gearing referred to by Mr. March Webb was put on by me, and was not in the ship when it came back from the West Indies. I mention this because Sir C. Bright's remarks would appear to imply that the whole machine as there shown was made to his instructions in the West Indies, and that thus I had claimed what I had no right to claim. I feel sure Sir Charles intended only to allude to the machine without the screw gearing, although he did not make this clear.

Mr. W. H. Preece: Mr. President, the first fact that strikes one on hearing this paper read is, that if this Society has done no other good, it has at least done this, that it has brought the contractors out of their shells; for instead of having all the operations of cable-laying kept a secret in the archives of their head offices, we have here a Member of one of the largest contracting firms in this country coming up and telling us all the secrets of the prison-house. We must be indebted not alone to Mr. March Webb for the very clear and able paper he has read, but also to Mr. Robert Gray and to the India Rubber and Gutta Percha Company for allowing Mr. Webb to do so. I had, with Col. Crossman, the very great pleasure of being a guest on board the *Dacia*, and having been often in more unfortunate circumstances on board ship in carrying out cable operations, I experienced very great pleasure in having a change once in my life and being on board an idle witness. Now, there were some things on board the *Dacia* that I liked very much. There were one or two things, perhaps, that I didn't like. I am bound to confess that I didn't fall desperately in love with the mode of laying shore ends by means of barrels. There may be circumstances when such *an operation* is absolutely necessary. There have been circumstances

when it would be impossible to lay the shore end of a cable by boats and when barrels would come into use; but from what I have seen I have not yet come to the conclusion that you can more rapidly and conveniently lay cables by barrels than by the ordinary method of forming a raft either by boats or steam launch. Mr. Phillips asked a question which I do not think was answered to his satisfaction, because I do not think that Mr. Robert Gray grasped the question in the same light as I did. I understood Mr. Phillips to ask whether there was not some probability of the weight of the cable sinking the barrels, and whether in consequence some inconvenience might not arise. But I do not think that at all likely to occur from the mode in which the operation is carried out, because the number of barrels, or the distance at which these barrels are placed apart, is simply dependent on the weight of the cable, and the buoyant power being known it is simply matter of calculation to say how many barrels should be required for any given length. When all the lines are pulled straight in the way described by Mr. R. Gray there is no more easy operation than simply to run along, cut away with a hatchet the lashes that fix the barrels to the cable, and so release them all one after the other. No barrel sinks, nor do I suppose it is at all possible in ordinary depths or with ordinary care that one should sink. Mr. Webb referred to one curious and anomalous operation connected with laying a shore end that probably did not strike the meeting, because he rather hurried over that part. He mentioned the fact that in laying one of the shore ends of the Chili and Peru cable a railway locomotive was employed for the purpose. We have heard of some very curious things in connection with submarine cables. I have heard of a submarine cable being broken by a bull! I have heard of another destroyed by fire! but I have not yet heard of a submarine cable being landed by a locomotive. The operation was, however, very simple. The locomotive was employed as power to pull ashore the hawser to which the barrels were attached. Another fact which struck me very forcibly during this expedition was the extreme value of the electric light. The electric light has been blazing away and trying to force itself into use in some of our large halls, and public places, and it has been *successful* certainly in the British Museum, whilst in other places it

has only been partially so; but certainly of all places where the electric light has been employed I know nowhere where it has shown itself so eminently useful and practical as on board a steam ship while laying a cable. In the operations connected with the sounding across the Mediterranean this was used all night long for several nights, and not one single sounding was lost. This light shone forth and was handled, thanks to Mr. Page's practical knowledge, with great skill, and it threw a flood of light from the quarter-deck that not only enabled the engineers to conduct their work, but the guests who were indulging in cards and cigars and a few of the luxuries on board the *Dacia,* deep into the small hours. During the paying-out it certainly was of enormous advantage, for, in the whole time, never once from the time the ship left Marseilles till it arrived off Algiers did we lose the benefit of what may be called "daylight." In the five or six days occupied I do not think one single minute was lost through the absence of a lamp or through the absence of light. It happened in the year 1860 that I read a paper in this very hall before the Institution of Civil Engineers on "The Maintenance and Durability of Submarine Cables," and one of the results of my experience, which at that time was not great, in maintaining cables was to advotate the absolute necessity of obtaining a clear and definite survey of the bottom of the ocean upon which the cable was about to be laid. I urged that with great force at the time, and the then President of the Institution asked me from his chair in rather a gruff tone whether I wished the bottom of the sea to be surveyed like the land for a railway. I said, "Certainly. I do not think that any cable should be laid on the bottom of the ocean until we know what the bottom of that ocean is"—and from that day to this year I have in more ways than one been striving to urge that principle. I do not think that a single deep sea cable has been laid yet where a *proper* survey of the ocean has been made. The Atlantic has been sounded but at distances of twenty miles. Plans have been drawn: sections have been shown of the Atlantic, and it has been generally assumed that we know a good deal of its bottom: but, supposing we were to take a line from John o' Groat's House to Land's End and drop a lead at every 30 miles, what should we know of the contour of the *country? So with* the reading and mere sounding at every 20 or

30 miles we get a very faint notion of what the bottom of the Atlantic or any ocean is. It, therefore, afforded me great satisfaction to find that, before Mr. R. Gray undertook to lay this Cable between Marseilles and Algiers, he was determined to devote the whole of the time at his disposal to finding out all he could of the bottom of the Mediterranean, and the ship was fitted with the apparatus that has been described to you. (The speaker here referred to a map, without which his remarks would not well be understood.) The result was, that for this line no less than 143 soundings were obtained, 93 of which were obtained by the *Dacia.* The apparatus worked perfectly. Nobody but those who have seen the operation of Sir William Thomson's sounding apparatus can conceive how easy it is to sound in the greatest depths. The ridiculous little wire that you fancy you could tear with your teeth runs out with the greatest regularity and the most wonderful speed. In fact, as the Paper said, it had taken soundings in 1,500 fathoms in only 25 to 30 minutes' time. The wire ran out at the rate of about 12 or 13 feet per second, and was brought up as quick. The wire, in fact, came up to the surface just as quickly as it fell, and more than that, it brought up the bottom with it. Mr. Webb did not refer to the fact that here are the identical sounding weights themselves. (Exhibited.) It is rather different to the sounding apparatus generally used. It is so constructed that it offers the same resistance to falling as it does to rising. Having described the manufacture of this apparatus, Mr. Preece went on to say—The result of these soundings was that we obtained 93 samples of the bottom. These have been very carefully placed in bottles, and I am hoping very shortly, with the aid of one of the most distinguished physicists and microphists, to be able to thoroughly examine the contents, and perhaps be able to bring them before the Society. From what I have been able to find out myself, I think that the bottom of the Mediterranean differs but very little from the bottom of the Atlantic, but there is this great peculiarity in the Mediterranean, that the temperature of the water at the bottom is very high. The Mediterranean appears to be a hot-water bath. The temperature of the water all over is uniform—about 55 degrees Fahr. The cable tests show it to be 54 degrees. The tests taken by Carpenter and others show

that the temperature is uniformly over the Mediterranean 54, while in the Atlantic it is about 34 degrees, and sometimes as low as 32 degrees, and thus you will see the great difference between the water in the Mediterranean and that in the Atlantic. A more perfect preservative for a cable could not be found than this stuff that comes up from the bottom of the Mediterranean, and when once a cable lies there, there is no reason why it should decay. In fact, the specimens upon that table which have been laid in this soft ooze show very little signs of decay indeed. The galvanisation is as perfect almost as when first laid down. When once a perfect cable has been laid in safety in this soft ooze, there is no reason why it should decay. With regard to testing operations, I can only say that I watched them with a good deal of interest, and found that everything worked perfectly and nicely; and although I have not had the pleasure of seeing other testing operations (of which we have heard a good deal) in actual operation, I have no doubt that in practice the operation described by Mr. Webb will be as effective in giving constant communication on the state of the cable between the shore and the ship as more celebrated ones that have emanated from a gentleman not very far from my left hand side (Mr. Willoughby Smith). I have no other remarks to make, but simply to say that if I find from the examination of the bottom of the Mediterranean which has been brought to London anything of any interest it will certainly be brought before the Society. Before sitting down I have only to express my deep obligations to Mr. Webb for his valuable paper, and to propose that a hearty vote of thanks be accorded to him for it.

Mr. SMITH: I have much pleasure in seconding it.

Mr. MARCH WEBB said he had no observations to make, and he simply returned thanks.

The CHAIRMAN: It is a very interesting paper, and by it he has gone far to remove the reproach said to attach to us by Mr. F. C. Webb, though I am bound to say I think he deserves it as much as anybody, for he has had a great deal of experience, and it was for him to give a paper on the subject.

The CHAIRMAN then announced the result of the ballot for Council and Officers to be as follows:—

COUNCIL 1880.

President.

W. H. PREECE, M.I.C.E.

Past Presidents.

Lieut.-Colonel J. U. BATEMAN-CHAMPAIN, R.E.	C. V. WALKER, F.R S.
CHARLES WILLIAM SIEMENS, F.R.S., D.C L.	LATIMER CLARK, C.E.
Professor ABEL, C.B., F.R.S.	SIR WILLIAM THOMSON, F.R.S., LL.D.
	FRANK IVES SCUDAMORE, C.B.

Vice-Presidents.

Professor G. C. FOSTER, F.R.S.	Major C. E. WEBBER, R.E.
CARL SIEMENS, M.I.C.E.	WILLOUGHBY SMITH.

Members.

Professor W. G. ADAMS, F.R.S.	CHARLES HOCKIN, M.A., C E.
W. S. ANDREWS.	Professor D. E. HUGHES.
WILLIAM T. ANSELL	LOUIS LOEFFLER.
SIR CHARLES BRIGHT.	C. E. SPAGNOLETTI, M.I.C.E.
H. G. ERICHSEN.	AUGUSTUS STROH.
Col. GLOVER, R.E.	C. F. VARLEY, F.R.S.

Associates.

ALEXANDER J. S. ADAMS.	Captain MACGREGOR GREER, R.E.

J. T. HILL.

Hon. Treasurer.	*Hon. Secretary.*
EDWARD GRAVES.	Lieut.-Col. FRANK BOLTON.

On the motion of Mr. ANDREWS, seconded by Mr. ERICHSEN, a vote of thanks was given to the Scrutineers for their services.

The Meeting concluded with a ballot, at which the following gentlemen were elected :—

Foreign Members.

Dr. Philipp Wilhelm Brix.	August Edward Granfeld.

Members.

Tom B. Andrews.	Edward Henry Bold.
Edward Charles Bird.	Sir James Carmichael, Bart.

K. E. Symons.

Associates.

Edwin Blakey.
Sergt. Arthur D. Bradshaw, R.E.
William Dunbar.
Frederic William Gifford.
John Stephen Hewitt.
Sergt. Samuel Hockey, R.E.
Chas. Streatfield James.
William Kingsland.
Elie Jean Lecoat.
John Loam.
John Errington Middleton.
Charles Edward O'Donnell.
Amedée Jules Emile Paul.
Frederick John Smith.
J. E. Whitaker.
Sergt. Charles H. Whittingback, R.E.
Thomas Wright

Student.

Arthur William Brackett.

REPORT

To the Council of the Society of Telegraph Engineers

ON THE

BIRMINGHAM WIRE GAUGE.

Your Committee having placed themselves in communicatio with many of the leading manufacturers and consumers of wir and makers of wire gauges, and with others interested in th subject, are of opinion—

1. That there is an urgent necessity and demand for a standar wire gauge, and that the custom recommended by some of definin the size in "mils," or thousandths of an inch, though perfectl satisfactory in any individual case, does not entirely supply th requirements of trade and commerce, which necessitate the use o a certain limited number of definite and well-known sizes.

2. That the gauge to be adopted should not vary materiall from the present gauges now in use under the name of th Birmingham Wire Gauge and other similar names, as these gauge have been based on long practice and experience, have becom thoroughly rooted in technical language, and are well adapted t the practical requirements of trade.

The introduction of an authorised standard gauge (using th word in the sense of a series of definite sizes) has long been ad vocated, and is exciting attention on the part both of manu facturers and consumers, and more recently of the Governmen and of the Chambers of Commerce. Many persons have written o

the subject, and several excellent gauges have been proposed, which would be perfectly satisfactory if only they could become universal. The movement gains strength every day, and is by no means confined to England. The manufacturers of America, France, Belgium, and Germany, who all more or less make use of the Birmingham Wire Gauge, are taking considerable interest in the question.

The Birmingham Chamber of Commerce, on the 24th October, 1878, addressed a circular letter to other Chambers of Commerce and manufacturers, requesting their opinion and advice on the subject, with a view to take united action, and more recently the Associated Chambers of Commerce have appointed a committee to examine and report on the subject.

The Board of Trade is also seeking information, and in the 12th Annual Report to Parliament on Standard Weights and Measures for 1877-78, the Warden of the Standards says :—

"In the last report reference was made to the Birmingham Wire Gauge used by engineers and those engaged in the metal trades in measuring wires and metal plates.

"This gauge (hereinafter referred to as the B. W. G.) is represented by a series of numbered slots or cuts on the edges of a small rectangular steel plate. It is the practice to distinguish the diameters of wires and the thickness of plates of metal by the number of the slot or cut which the wire or plate may fit.

"There is no standard of such gauge or common agreement amongst those interested as to what are the dimensions in parts of an inch of the several slots or sizes of the true B. W. G. Its sizes are not geometrically or arithmetically progressive, and, consequently, bear no definite relation to each other. Its origin is obscure, and it would appear that the several slots or sizes arose from time to time as a new wire or new plate was introduced, and as the exigencies of a particular trade demanded. Considerable annoyance to engineers and pecuniary loss to contractors is stated to occur from a want of accuracy in the copies of this gauge, and the necessity of establishing a standard has lately been discussed, both in this country and in the United States.

"In Germany gauges for wire or sheet iron have not yet been officially controlled. Independently of a number of gauges differing from each other in use in the various manufacturing districts, the Birmingham Wire Gauge, commonly called the 'English Gauge,' is mostly in use in Northern Germany for measuring sheet iron, wire, and hoop iron. In Southern Germany the Birmingham Wire Gauge is also used, and for the measurement of wires the French gauge, which is a progressive scale of tenths of a millimetre (1 millimetre = 0·0393709 inch), is also used. For sheet iron the 'Dillingen Gauge,' which is a scale of Paris lines (1 line = 0·08881377 inch), is also used in Southern Germany. The wire factories in Westphalia use a particular gauge called the 'Bergish or Westphalian,' the sizes of which, like the B. W. G., are established by arbitrary progression. For some time past the question of establishing a uniform wire gauge, and a uniform numbering of wires, has been energetically agitated in Germany.

"General Gloukhoff states that in Russia there is no standard gauge for wire and sheet metal. The manufacturers in Russia use different gauges of English, German, and French patterns.

"In Canada only one gauge is known to mechanics—the Birmingham Wire Gauge made by Stubs, of Warrington.

"In France measurements are made by the scale of one-tenth of a millimetre, as well as by the Birmingham and Dillingen arbitrary gauges.

"In America the B. W. G. is extensively used. From the information kindly forwarded to this department by Professor Hilgard, it would appear also that the wire gauge made by Messrs. Browne and Sharpe, of Rhode Island, and adopted by the brass manufacturers in 1858, is also used in the United States; this gauge has, however, no official sanction. A committee of the American Institute recently reported on this subject (December 22nd, 1877), and they recommended the adoption of the system of expressing sizes in thousandths of an inch, as in the Whitworth Gauge, or in fractions of a millimetre.

"It appears from this information that the B. W. G. is extensively used abroad as well as in this country, and that efforts

are being made to secure more uniformity in the measurements of wires and sheet metal.

"Although the present B. W. G. appears to be sufficiently accurate for the workman's purpose, it is stated to be ill adapted for the use of engineers and others demanding accurate measurements. The efforts which have been made by Sir Joseph Whitworth, and recently by Mr. Latimer Clark and Mr. C. V. Walker, and other authorities, to introduce a better system of measurement, indicate the necessity of some regulation of these trade measures. Proposals have been made from time to time to introduce a new and corrective gauge, calculated on such a scale as would correct the irregularities of the present B. W. G. by introducing a series of sizes progressing arithmetically or in aliquot parts of an inch.

"No corrective scale of numbers and sizes has, however, been yet made known, which does not differ so appreciably from the present B. W. G. as to render its acceptance very doubtful by those who have to handle such instruments. To set up a new gauge would only be to add to the confusion already existing.

"In 1846 Mr. Holtzapffel ascertained the dimensions in parts of an inch of good specimens of the B. W. G. of that time. The general acceptance of his measurements, and their practical agreement with the experience of this department, indicates a basis upon which it would be possible to construct a standard of this gauge.

"This department, however, could not recommend that an arbitrary measure like the B. W. G., notwithstanding its present extensive use, should be legalised as an imperial secondary standard measure. In any contract, bargain, sale, or dealing, the sizes of wires and metal-plates are legally expressed only in imperial measure or in parts of an inch.

"So long as manufacturers continue to use an arbitrary measure like the B. W. G., whose accuracy cannot admit of verification, so long will differences exist in the relative sizes of their productions."

In reference to these remarks your Committee fully admit that

so long as manufacturers continue to use a diversity of measures, like the various gauges now in use, so long will differences exist in the relative sizes of their productions; but they do not at all agree with the opinion that to set up a new gauge would only be to add to the confusion already existing. On the contrary, they are clearly of opinion that it is the only practicable remedy for the present state of chaos which prevails. It appears to them most desirable to establish a legalised gauge, whether new or old, to give it a distinctive name, and to leave it by its own inherent accuracy and authoritative character to supplant its rivals.* A large number of intelligent manufacturers and consumers would at once adopt it, either from a conviction of its utility or a desire to be enabled to specify their wants with accuracy. The gauge would soon become a necessity in every workshop, and from its precision it would necessarily and rapidly supersede the numberless other gauges at present in use. To ensure this result it is only necessary that the gauge should not differ materially from the ordinary Birmingham Wire Gauge.

Your Committee have not been able to obtain precise information as to the origin and history of the Birmingham Wire Gauge. It is the opinion of those best informed that it originated among the wire drawers, and was probably based on a series of drawn wires—No. 1 being the original rod,† and the succeeding numbers corresponding with each draw of the wire—No. 10, for instance, having passed ten times through the draw plate. In the existing gauge each size weighs on the average about 20 per cent. (19·9) less than the preceding size, this reduction having been found the most suitable in practice, especially at a time when steam power was unknown. This ratio still remains true to a great extent, both for large and small wires, being dependent on the physical properties of the metals employed, that is to say, the relation between their cohesive strength and the resistance opposed by the

* The system of electrical units of the British Association is an instance how soon such an authorised standard becomes adopted.

† In the Birmingham Wire Gauge the difference between No. 1 and No. 2 is smaller than that between other sizes, probably owing to its being the first draw from the rolled rod.

draw plate. In this way a thoroughly practical series of sizes has been determined, and no gauge which does not approximate to these sizes would be likely to commend itself to the wire drawing trade.

The present B. W. G., or Birmingham Iron Wire Gauge, was certainly well known at and before the beginning of the present century, but no authoritative copy or table of the original gauge is known to be in existence.* Mr. Glazebrook Rylands, of Warrington (whose letter appears in the Appendix), Mr. Johnson, of Manchester, and Mr. James Robinson, of Birmingham, and others, have had very old gauges in their possession—one bearing a date of 1795, but too much worn to be of value. Mr. Hughes in his pamphlet gives the sizes of several very old gauges, and shows that the ordinary numbers of wire were in common use in 1735, and earlier. There is strong evidence to show that when numerical values were first assigned to the sizes they were given in 32nds and 64ths of an inch, and the oldest tables were in that form. Since Mr. Holtzapffel's measurements were made the sizes have been usually given in thousandths of an inch. The different gauges now in use might be counted by hundreds. In the absence of any standard, every manufacturer and every dealer feels himself at liberty to choose, or even to construct, his own wire gauge. Every wire drawer has gauges adjusted to suit special objects. Where competition is keen, wire is commonly drawn by one gauge and sold by another; half sizes and quarter sizes are in constant use among the dealers, the wire being sold as whole sizes. Sometimes four or five different gauge plates have been made for one maker—some by which the workmen are paid, and others by which the wire is sold. As many as seven different sized gauges have been made between the sizes 20 and 21. In the sheet gauges one maker makes a certain number of sheets to weigh 56 lbs., and calls them No. 40 gauge; another competitor makes the same number of sheets to weigh 85 lbs., and still calls them No. 40.

* By far the most complete account of the wire gauge is to be found in a pamphlet entitled, "The English Wire Gauge, with Descriptive Tables and Drawings," by Mr. Thomas Hughes, of Greenfield, Holywell, Flintshire. Spon & Co., London, October, 1879.

The whole system is in confusion, and lends itself to those who desire to use fraudulent practices.*

It seems to be the universal opinion that an authoritative standard is urgently required, though it might at first meet with considerable opposition, both from those who have been long accustomed to their own gauges (which they have been taught to consider authentic), and from those who have found a certain advantage in the vagueness of the present gauges. By the great mass of manufacturers and consumers such a standard would certainly be welcomed, provided it did not differ too greatly from the existing gauges. The numbers now in use represent, within certain limits, well known sizes, which are as familiar to the workman and tradesman as the letters of the alphabet, and these sizes are well known abroad as well as in England.

Up to the present time half and quarter sizes have not come into general use among consumers, in fact, such sub-divisions would be useless in the state of confusion which at present prevails, but in a properly constructed gauge they would be of great value, and they ought to be as determinately fixed as the other sizes. If the present gauge were uniformly sub-divided the quarter sizes would represent on an average a decrease of about 5 per cent. in weight in each quarter size of wire, or a decrease of about 2½ per cent. in each quarter size in the weight of plate. Such a gauge would

* Mr. Hughes says:—"An order was received by cable from New York for metal No. 36 wire gauge; the receivers gave out the order accordingly. The manufacturers, discovering that the order was for America, rightly concluded that Stubs' Gauge was intended. Had the order been executed No. 36 Birmingham Wire Gauge, the metal would have been useless for the purpose intended. No. 36 on Stubs' Gauge is No. 44 on the Birmingham Wire Gauge, and the difference in price of the metal on this order £26 per ton. Many disputes have taken place in different parts of the United Kingdom as to the correctness of wire gauges. In the same town, some use Stubs', some the Warrington, some the Lancashire, some the Yorkshire, some the Birmingham, some the iron wire gauge, and some their own made wire gauge—all maintaining the gauge in their own possession to be the correct one. Some keep several of the different gauges on their premises, and, knowing their variations, take advantage of published trade lists. When the metal is delivered it is refused, being too thick or too thin. The manufacturer has either to submit to a substantial reduction in price—what the customer aimed at—or suffer a greater loss by replacing it."

appear perfectly suited to take the place of the various plate and other gauges in common use.

We proceed now to describe a few of the most important gauges, which naturally divide themselves into two classes, viz.:—

1. The Empirical Gauges.

2. The Geometrical Gauges.

1. The Empirical gauges are those in which the gradations between the respective sizes are formed by arbitrary differences, and include nearly all the forms of gauges at present in use. The sizes being arbitrarily determined they have the advantage that they can be expressed by an even number of "mils" or thousandths of an inch. This advantage is, however, to some extent counterbalanced by two drawbacks. It is impossible to express them in metrical measures without the use of many decimals, and this, since English measures are never employed on the Continent, constitutes a certain barrier to their use internationally.

2. No dimension less than a thousandth of an inch being employed, the intervals between the very small sizes become proportionately great, thus, 3 mils and 2 mils (two sizes which are in extensive use by electricians for resistance coils) differ from each other by more than 100 per cent. in weight and in electrical resistance. The next sizes, 4 mils and 3 mils, differ by 56 per cent., the difference becoming smaller as the sizes increase.

The Geometrical gauges are formed by perfectly uniform decrements of weight from size to size, the difference being the same in the smaller sizes as in the larger. To express these sizes accurately it is necessary to resort not only to mils, but to decimal parts of mils. In practice, the tenth of a mil, or $\frac{1}{10000}$ of an inch, is the minutest difference that can be conveniently measured, for that, only one decimal place is required, even in the finest sizes. They have the merit that they have no special relation to English measures, and are, therefore, as well suited for adoption by continental nations as by our own. The half and quarter sizes can also be defined with absolute precision.

1. *Stubs' Gauge*—sometimes called the Warrington or Lan-

cashire Gauge.—This is one of the best known and most extensively used. In 1843 the late Mr. Charles Holtzapffel, in conjunction with Mr. Peter Stubs, of Warrington, accurately determined the sizes of the best gauges then in Mr. Stubs' possession, and formed a table of them, which was published in Mr. Holtzapffel's work on Turning.* To these sizes Mr. Stubs has since adhered. His gauges have also been extensively copied by the Warrington and Lancashire tool-makers, and by those of Sheffield, during the last forty-five years, and have been distributed in large quantities throughout the world. A table of the sizes is given below, with those of other makers, and though very irregular in many of its gradations, this gauge would, if it had become universal, have satisfied the requirements of commerce. It may, perhaps, be considered the most authoritative gauge in common use.

Mr. Stubs has also furnished the Committee with tables of his steel wire and letter gauges, which differ materially from the iron wire gauge.

2. *Wynn's Wire Gauge.* — Messrs. W. & C. Wynn & Co., of Birmingham, have manufactured large numbers of gauges, which have been sent to all parts of the world. They have adhered pretty closely to the same sizes for the last 70 or 75 years, and they believe the gauge they use has been in existence more than a century, and that it had the same origin as Stubs' Gauge, which it very closely resembles. They introduced slight modifications in some of the sizes many years ago, and the result is that it is remarkably regular in its gradations, and might, but for its age, have been regarded as an improved Stubs' Gauge. Messrs. Shakespear & Co. also manufacture gauges in the same neighbourhood, but there is nothing in their sizes to call for remark.

3. *Cocker's Gauge.*—Mr. James Cocker, of Liverpool, published, in 1858, a table of proposed new sizes for the Birmingham Wire Gauge,† in which he gives a series of sizes, decreasing at first by 40 mils, and then by gradations of 20, 10, 5, 3, 2, and 1 mils. The decrements are fairly symmetrical, and the numbers,

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* Holtzapffel, "Turning," London, 1846, vol. 2, p. 1013.

† James Cocker's Tabular Decimal Scale of proposed New Sizes for Wire. *Liverpool*, 1858.

consisting mainly of tens and fives, are not difficult to commit to memory. Moreover, the gauge nowhere departs very widely from the Stubs', Wynn's, and other gauges in ordinary use, and it would, if adopted generally, satisfy all requirements. Mr. Cocker proposes, in the same paper, rectified sizes for the sheet gauge, the needle wire gauge, the Lancashire and letter wire gauge, the music wire gauge, and the screw wire gauge. None of these, however, appear to have come into use, though they bear evidence of much care and thought. Mr. Cocker was also the inventor of a very ingenious and useful form of excentric wire gauge for measuring the diameter of wires, and was the first to employ the word "mil," as signifying a thousandth of an inch, an innovation of such manifest value and convenience that this unit might with great public benefit be made a standard measure of the Board of Trade.

4. *Rylands' Wire Gauge.*—Mr. Glazebrook Rylands, of Warrington, has given great attention to the subject, and the results are given in an interesting letter to Mr. Latimer Clark, which is appended to this report. Up to the year 1862 he employed a gauge differing little from those previously described; but about this period he carried out an extensive series of experiments on the subject, and also obtained possession of some old Birmingham gauges, dating back to 1810 or earlier, and was induced, in 1862, to issue a table of sizes based on practical experiment, which is given below, and in which the sizes, especially in the earlier numbers, are considerably smaller than in the ordinary gauges, though starting, like the others, from the usual basis of No. 1 = 300 mils. It will be seen hereafter that all the ordinary gauges are characterised by having small per centage decrements in the larger, and larger ones in the smaller sizes. This may have arisen from a deficiency of power in the days when hand labour was much employed. Mr. Rylands' experiments led him to give more rapid decrements in the larger sizes, and is therefore more uniform throughout.

5. *The Old Birmingham Wire Gauge.*—A great variety of gauges are in use under this and other names which have no feature of special interest. Nor is it necessary to allude further

to the various needle wire, brass wire, piano wire, letter and other gauges, which are given in Holtzapffel's work, in Sir Joseph Whitworth's paper, and elsewhere.

6. *Walker's Gauge.*—We now come to a group of gauges closely affiliated to the others, but in which the sizes are defined in the ordinary fractions of 8ths, 16ths, 32nds, and 64ths of an inch, which was probably the original form of the Birmingham Wire Gauge. One of these forms was described in a paper read before the Society of Telegraph Engineers in 1878.* A duplicate of the gauge plate in question had been presented to the Astronomer Royal as a standard in 1860 by Mr. Richard Johnson, of Manchester, and was constructed by the late Mr. Thomas Mallinson, of Birmingham, but its origin is otherwise unknown. In addition to the notches on the gauge itself, it has engraved on the case the dimensions of the sizes in fractions of an inch. The differences decrease with great regularity, at first by 8ths, then by 16ths, 32nds, 64ths, and into smaller sizes by 640ths of an inch, but still with equal regularity. With all this the gauge does not differ materially in sizes from the ordinary Birmingham Wire Gauges, except in the higher numbers, which are much too large, and as Mr. Walker supposes, it may probably represent very closely the original form of the Birmingham Wire Gauge. As the 640th of an inch is too large a unit of decrement for the smaller sizes, Mr. Walker proposes in some cases a further sub-division of this unit by half. Notwithstanding its apparent claim to authenticity it does not seem suitable for general adoption, as its symmetry is lost by conversion into decimal fractions, and it is then not superior to the ordinary gauges. There would be a difficulty in the formation of half and quarter sizes, and it would be quite unsuited for adoption in continental countries on account of its English mensuration.

7. *Watkins' Gauge.*—Mr. John Watkins, of Birmingham, has published a table of the Birmingham Wire Gauge,† with the sizes given both in thousandths and in common fractions of an inch. In the latter form it is in most of the sizes identical with Mr. Walker's

* *Journal of the Society of Telegraph Engineers*, vol. 8, page 215.

† "Correct value of the Birmingham Wire Gauge," by John Watkins, Birmingham. London, 1878. Spon & Co.

Gauge. Mr. Watkins' investigations are of recent date, and made quite independently of others. By inquiries among practical wire drawers he has convinced himself that the original measures were made in 8ths, 16ths, 32nds, and 64ths of an inch, and has arrived at results which are very similar to those given by Mr. Walker, and which chiefly differ from them in the smaller sizes. It has, however, one peculiarity in that it has an additional size, No. 3½, interpolated among the others, which is exactly ¼ inch in diameter, answering to Mr. Walker's size of No. 3.

8. *South Staffordshire, or Robinson's Gauge.*—Mr. James Robinson, of Birmingham, has made a very extensive series of inquiries on the subject, in the neighbourhood of Birmingham and elsewhere, and has prepared a table of sizes from actual measurements, with the intention of laying it before the Birmingham Chamber of Commerce. In most of the sizes it closely resembles the Watkins and Walker Gauges, and bears strong evidence of having been formed on the fractional system of 8ths and 16ths, &c. In the higher numbers both it and Mr. Watkins' Gauge follow the ordinary Birmingham Wire sizes much more closely than Mr. Walker's Gauge. Mr. Robinson's table is called "Measurements of the South Staffordshire Sheet Iron Gauge," and he has the intention of publishing it when his inquiries are completed. He adds: "This gauge has been in use here over 50 years; it is without doubt the gauge of the iron trade. No other is in general use, and it is inapplicable to any other purpose. This gauge decides all disputes; there is but one maker of these gauges. It is unknown out of the district."

The gauges thus far considered have all apparently a common origin, and all agree more or less in their general sizes, and by averaging a number of them, and smoothing the differences, it might be possible to form a curve which would fairly represent them all, and give a combination gauge. This has been attempted, but its variations are still irregular, when if shaped into a flowing curve it departs rather widely from some of the sizes in general use.

9. *Messrs. Mallock & Preece.*—Messrs. Mallock & Preece, early

in 1872,* proposed a gauge for iron wire used for telegraphic purposes, based on regular increments of weight. They showed that wire is purchased, transported, and distributed by weight. Its breaking strain varies directly with its weight. Its resistance varies inversely with its weight, and therefore its capacity for carrying messages is directly dependent upon its weight. Weight is practically invariable in all temperatures and latitudes. They took as unit a wire weighing 25 lbs. per mile, and increased the gauge by regular increments of 25 lbs. This formed a series very closely approaching the Birmingham Wire Gauge. Wire purchased for the British Post Office, the Indian Government, and the Crown Colonies is based on this gauge.

It has, however, the great disadvantage of being applicable to only one material and one form, and it is not recommended for an authorised standard.

10. *American, or Brown & Sharp's Gauge.*—We come now to consider the Geometrical gauges, which are formed on the principle of a regular geometric progression, each size varying both in size and weight and in electrical conductivity by a uniform per centage on the principle of compound interest. They carry out scientifically the principle which underlies all the ordinary gauges in which "a sort of attempt has been made to have the differences of thickness which follow each other in some manner proportionate; they are the guess of the workman at a geometric proportion of dimension." Messrs. Brown & Sharp, of Providence, Rhode Island, in the United States, were the first to propose a geometric gauge about the year 1855. They established a regular progression of 39 steps between the English sizes No. 0000 and No. 35, taking these values at 460 mils and 5 mils, respectively. Their curve is formed by multiplying each diameter by ·890522 (or deducting 10·9478 per cent.) to form the next succeeding size. This gauge was named the American Gauge, and has been largely made by the proposers, on account of their high reputation for accuracy of workmanship, though it has not met with reception

* *Journal of the Society of Telegraph Engineers*, vol. 1, p. 79.

beyond the limits of their trade influence.* The sizes are so very much smaller than those of the Birmingham Wire Gauges that the greatest inconvenience would arise from its introduction, though it is in all other respects an excellent gauge.

11. *Clark's Gauge.*—In 1867 Mr. Latimer Clark proposed a geometrical gauge in which the intervals are so arranged that each size is exactly 20 per cent. less in weight (and in electric conductivity) than the preceding size.

He read a paper on this gauge at the meeting of the British Association in Dundee in 1867, and another at Exeter in 1869, both of which have been reprinted in the Journal of this Society.† By a slight modification of the starting point of this gauge, making the No. 0 size equal to 1 centimetre in diameter, this gauge follows very closely the sizes of the ordinary Birmingham Wire Gauges, while it has the advantages common to all geometrical gauges that it can be extended indefinitely in either direction for larger and smaller sizes; that a person knowing the weight or conductivity of any one size can readily calculate the others; that the differences are perfectly uniform throughout; that the half and quarter sizes can be defined with perfect precision, and that it is equally suited for English or Foreign systems of mensuration. Its objections are that even when defined in mils it requires in practice at least one decimal place—that the larger sizes and the smaller sizes are both larger than those of the ordinary Birmingham Wire Gauges, frequently differing from them by more than a whole size, though the middle sizes coincide pretty closely.

12. *Briggs' Gauge.*—In 1877 Mr. Robert Briggs, C.E., of Philadelphia, proposed a geometrical gauge based upon a reduction in the diameter of each size of one-tenth, and taking the same starting-point as the gauge last described, viz.,—No. 0 = 1 centimetre.‡ The difference between the two gauges is that Mr. Briggs' diminishes his sizes by 10 per cent. in diameter, or 19 per cent. in weight, and Mr. Clark by 10·557 per cent. in

* *Journal of the Franklin Institute*, December, 1877.

† *Journal of the Society of Telegraph Engineers*, vol. 7, p. 336.

‡ *Journal of the Franklin Institute*, December, 1877, "A Decimal Gauge for Sheet Metal and Wire," by Robert Briggs, C.E.

diameter, or 20 per cent. in weight. Mr. Briggs' sizes coincide approximately with the Birmingham Wire Gauges between the Nos. 6 and 13, but in all other parts of the scale they differ by one, two, or three sizes. It has not yet come into practical use.

In all other respects the gauge has all the advantages common to the geometric gauges. In this paper Mr. Briggs treats the subject of gauges at some length.

13. *Whitworth's Gauge.*—Sir Joseph Whitworth proposed in 1857* the gauge which bears his name, which—like everything from the same hand—bears the stamp of great mechanical precision. He names each size after the number of mils or thousandths of an inch in its diameter. As there would in the wires in ordinary use be on this system some four or five hundred different sizes, he suppresses the larger number and employs only certain selected sizes, diminishing by 40, 20, and 10 mils in the larger sizes, and by 5, 2, and 1 mils in the smaller sizes, and in this way he forms a table of 62 sizes in the whole; there is, however, this peculiarity —that his large sizes, instead of being No. 0 or No. 1, bear very high numbers, such as 375 or 400, while his finest size is called No. 1. It is, therefore, a complete reversal of all the other gauges, and bears no relation to the natural reduction of the draw plate. From this cause it does not meet the requirements of the wire drawing trade, and has not come into general use, and though excellent in itself it could not now be introduced without great opposition. Its great merit is its precision, it is, in fact, a curtailment or limitation of the system of expressing measurement in mils or thousandths of an inch which is in such universal use, and which must remain as the ordinary standard of last resort. Since half and quarter sizes are necessary, the symmetry of the system would, however, have to be broken, and it would tend to revert to the simple use of measurement by mils. Being an English gauge, it could not be adopted in foreign countries.

* "On Standard Decimal Measures of Length," by Sir Joseph Whitworth, Proceedings of the Institution of Mechanical Engineers at Manchester, June 25, 1857. Birmingham, 1858. This paper contains some valuable tables of other wire gauges and standard screw sizes.

14. *Hughes' Gauge.*—Mr. Thomas Hughes, of Holywell, in the pamphlet before alluded to,* has quite recently proposed an empirical gauge, formed by taking a mean of the other gauges. It of course does not differ greatly from the gauges in general use, though not formed on any scientific principle, and would, if adopted, form a good practical gauge; but its gradations are not symmetrical. The half and quarter sizes are not given.

Your Committee have not been called upon to report on the instruments used for measuring wire, &c., but on this point they would refer to Mr. Hughes' pamphlet, and to the "Report on a Standard Wire Gauge" — a paper read before the American Institute of Mining Engineers, and published in the *Journal of the Franklin Institute*, February, 1878. Reprinted in the *Journal of the Society of Telegraph Engineers*, vol. viii. p. 344, 1878.

It now becomes necessary to consider which form of gauge is best suited for recommendation as a standard gauge. Shall one of the existing Birmingham Wire Gauges be adopted as it stands; shall an attempt be made to perfect one of them by adjusting certain sizes, or (as Mr. Hughes has done) to form an average gauge from among them all; or shall the question be treated scientifically and some form of geometrical gauge be adopted?

If any of the existing or proposed empirical gauges be adopted, it would appear that Hughes', Wynn's, and Cocker's have the most uniform gradations, and most nearly conform to the average sizes. At the same time they do not appear to have met with any special favour on that account.

The Committee are on the whole of opinion that the best form of gauge for adoption as a standard would be one of the geometrical gauges already described, which are based on truly scientific principles. Among these the American Gauge unfortunately differs so widely in the principal sizes from the Birmingham Wire Gauges as to be inadmissible on the score of inconvenience,

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* "The English Wire Gauge" By Thomas Hughes. London, October, 1879. Spon & Co.

and the choice is therefore limited to Clark's and Briggs', or to some new form. Both of these start from the same basis, viz., No. 0 = 1 centimetre.

In Clark's system the weight of each size diminishes by 20 per cent., and consequently it increases by 25 per cent.; the diameter or thickness diminishes by 10·557 per cent., and therefore increases by 11·803 per cent.

In Briggs' system the weight of each size diminishes by 19 per cent., and therefore increases by 23·456 per cent.; the diameter or thickness diminishes by 9 per cent., and therefore increases by 10 per cent.

So far as these considerations have any weight, Clark's system would appear better suited for a wire gauge, and Briggs' for a plate gauge, on account of the greater simplicity of the relations in each case. But these considerations are not really of any importance whatever, for in reducing the weight or thickness of either wire or plate by more than one number, we are at once driven into a number of decimal places whichever system be adopted, and the same is true of all other systems, actual or proposed, and as a matter of fact all such weights will in practice always be taken from ready constructed tables.

A question of far more importance is, which of these gauges conforms the more closely to the existing Birmingham Wire Gauges. On this point the superiority rests decidedly with Clark's system, and this gauge is therefore the one recommended for adoption as a standard gauge. It would be desirable that it should bear some distinctive name, such as British Standard Gauge (B. S. G.), or Centimetre Gauge; some remarks on its merits and demerits have already been made. In considering the effects of its introduction as a standard gauge, it may be observed that upon the whole it differs from the existing gauges scarcely more than they differ among themselves, and since its introduction would be gradual, no new inconvenience would be felt beyond that at present experienced.

For a certain time many would continue to adhere to the various gauges as hitherto, a certain number would adopt the standard gauge, which would, therefore, become a necessity in

every workshop. In this way the workmen and dealers would gradually become acquainted with it, and would soon begin to prefer it on account of its precision and uniformity, and its authority as a gauge of last appeal, and it would gradually, but surely, supersede all other gauges. In the large 0 sizes, and up to No. 4, it differs from the Birmingham Wire Gauges by at least a whole size; this being owing to the small proportionate reduction of size in the earlier number of the old gauges. This fact would, however, become fixed in the memory of the workman in a few minutes, and he would soon become as familiar with the difference between British Standard Gauge and Birmingham Wire Gauge as he is with the variations in the many gauges he now has to work to. From No. 5 to No. 18 the variation is never equal to a whole number, and these sizes include the great bulk of the wire manufacture.

From No. 20 to the smallest sizes the difference is generally equal to about one size (for example, No. 22 would become known as No. 23), but these sizes are in less demand, and the variations in the existing gauges are equally excessive.

By the use of half and quarter sizes we have a series of great uniformity, varying about 2½ per cent. in weight of plate, or diameter of wire, and about 5 per cent. in weight of wire, and the gauge would, therefore, appear to be well suited for a plate gauge, and especially in the cases of very thin sheets, as there are no sudden jumps in any part of it.

Tables of weight of wire and sheet for different metals would become possible, and would enable any of the sizes to be bought and sold by weight, or the thickness checked by their weight. Concurrently with this, the system of measurement by thousandths of an inch will be available as heretofore, and in the case of a few of the large consumers, who have wire drawn for their own special consumption, it may still continue to be preferred.

The standard gauge can, of course, be readily extended in either direction without limit, and as it is quite independent of all systems of mensuration, though based on the centimetre, it is likely to be adopted by continental nations as well as in the United States.

The following tables are appended :—

1. A table of the various gauges previously described.

2. A table giving the per centage difference or reduction in weight between the several sizes, intended to illustrate the comparative irregularity of their gradation.

3. A table showing the per centage difference in weight between each size and the corresponding size of Stubs' Gauge. This gauge, though one of the most irregular, is chosen because it represents a fair sample of the gauge at present in use.

4. A table of the proposed Standard Gauge in millimetres.

5. A table of the proposed Standard Gauge in mils.

6. A specimen table of the half and quarter sizes,

F. A. ABEL.
LATIMER CLARK.
WALTER HALL.
J. THEWLIS JOHNSON.
W. H. PREECE.
C. W. SIEMENS.
WILLOUGHBY SMITH.
C. V. WALKER.

TABLE OF VARIOUS GAUGES,

In mils = $\frac{1}{1000}$ of an inch.

No of Gauge.	Stubs.	Wynn.	Cocker.	Rylands, May, 1866.	Walker.	Watkins.
000	425	375	420	...	437·5	437
00	380	350	380	...	375	375
0	340	325	340	326	343·8	343
1	300	303	300	300	312·5	312
2	284	280	280	274	281·3	281
3	259	258	260	250	250	265
4	238	238	240	229	234·4	234
5	220	218	220	209	218·8	218
6	203	200	200	191	203·1	203
7	180	182	180	174	187·5	187
8	165	165	160	159	171·9	171
9	148	149	140	146	156·3	156
10	134	134	130	133	140·6	140
11	120	120	120	117	125	125
12	109	107	110	100	112·5	109
13	95	95	100	90	100	93
14	83	84	90	79	87·5	78
15	72	73	80	69	75	70
16	65	65	70	62·5	62·5	62
17	58	58	60	53	56·3	54
18	49	50	50	47	50	46
19	42	43	45	41	43·8	42
20	35	37	40	36	37·5	38
21	32	32	35	31·25	34·4	34
22	23	29	30	28	31·3	31
23	25	26	25	...	28·1	28
24	22	23	22	...	25	25
25	20	21	20	...	23·4	22
26	18	18·5	18	...	21·9	19
27	16	16	16	...	20·3	17
28	14	14	14	...	18·8	15·6
29	13	13	12	...	17·2	13
30	12	12	11	...	15·6	11
31	10	11	10	...	14·1	9
32	9	10	9	...	12·5	7

TABLE OF VARIOUS GAUGES—(*continued*),
In mils $= \frac{1}{1000}$ of an inch.

No. of Gauge.	South Staffordshire.	Mallock & Preece.	American.	Clark.	Briggs.	Whitworth.	Hughes.
000	...	...	409·6	492·1	486·1	...	450
00	...	...	364·8	440·2	437·5	...	400
0	...	...	324·9	393·7	393·7	...	350
1	302·5	294	289·3	352·1	354·3	1	300
2	275·5	275	257·6	315	318·9	2	280
3	256·5	255	229·4	281·7	287	3	260
4	236	232	204·3	252	258·3	4	240
5	217	220	181·9	225·4	232·4	5·	220
6	207·5	195	162	201·6	209·2	6	200
7	184·5	180	144·3	180·3	188·3	7	180
8	167·5	164	128·5	161·3	169·5	8	165
9	153	147	114·4	144·2	152·5	9	150
10	134	134	101·9	129	137·3	10	135
11	116·5	120	90·7	115·4	123·6	11	120
12	106·5	104	80·8	103·2	111·2	12	110
13	96·5	95	72	92·3	100·1	13	100
14	89	85	64·1	82·6	90·1	14	85
15	73	73	57·1	73·9	81·1	15	75
16	60·5	60	50·8	66·1	73	16	65
17	54	...	45·3	59·1	65·7	17	58
18	49·5	...	40·3	52·8	59·1	18	50
19	41·5	42	35·9	47·3	53·2	19	44
20	39	...	32	42·3	47·9	20	38
21	34	...	28·5	37·8	43·1	...	34
22	28·5	...	25·3	33·8	38·8	22	30
23	26	...	22·6	30·2	34·9	...	26
24	23	...	20·1	27·1	31·4	24	23
25	19·5	...	17·9	24·2	28·6	...	20
26	16·5	...	15·9	21·6	25·4	26	18
27	15·5	...	14·2	19·4	22·9	...	16·5
28	14·5	...	12·6	17·3	20·6	28	15
29	11	...	11·3	15·5	18·5	...	14
30	10·5	...	10	13·9	16·7	30	13
31	10	...	8·9	12·4	15·0	...	11·5
32	9·5	...	7·9	11·1	13·5	...	10

TABLE SHOWING THE PERCENTAGE REDUCTION IN WEIGHT FROM EACH PRECEDING SIZE.

No. of Gauge.	Stubs.	Wynn.	South Staffordshire.	Cocker.	Walker.	Whitworth.	American.	Clark.	Briggs.	Hughes.
000	12·4	...	...	...	23·4	...	20·7	20	19	...
00	20·1	12·9	...	18·1	26·2	...	20·7	20	19	20·99
0	19·9	13·8	...	19·9	16·0	...	20·7	20	19	23·44
1	22·1	13·1	...	22·1	17·4	...	20·7	20	19	26·53
2	10·4	14·6	17·1	12·9	19·0	300	20·7	20	19	12·9
3	16·8	15·1	13·3	13·8	21·0	125	20·7	20	19	13·78
4	15·6	14·9	15·3	14·8	12·1	78	20·7	20	19	14·79
5	14·5	16·1	15·4	16·0	12·9	56	20·7	20	19	15·97
6	14·9	15·8	8·5	17·4	13·8	44	20·7	20	19	17.35
7	21·4	17·2	20·9	19·0	14·8	36	20·7	20	19	19·00
8	16·0	17·8	17·3	21·0	15·9	31	20·7	20	19	15·97
9	19·5	18·4	16·9	23·4	17·4	27	20·7	20	19	17·35
10	18·0	19·1	23·3	13·8	19·0	23	20·7	20	19	19·00
11	19·8	19·8	24·4	14·8	21·0	21	20·7	20	19	20·98
12	17·5	20·5	14·5	16·0	19·0	19	20·7	20	19	15·97
13	24·0	21·2	17·9	17·4	21·0	17·4	20·7	20	19	17·35
14	23·7	21.8	14·9	19·0	23·0	16·0	20·7	27	19	27·75
15	24·7	24·5	32·7	21·0	26·5	15·1	20·7	20	19	22·15
16	18·5	20·7	31·3	23·4	30·5	13·8	20·7	20	19	24·89
17	20·4	20·4	20·3	26·5	19·0	12·9	20·7	20	19	20·38
18	28·6	25·7	16·0	30·6	21·0	12·1	20·7	20	19	25·69
19	26·5	26·0	29·7	19·0	23·4	11·4	20·7	20	19	22·56
20	30·6	26·0	11·7	21·0	26·6	10·8	20·7	20	19	25·41
21	16·4	25·0	24·0	23·4	16·0	10·2	20·7	20	19	19·94
22	23·4	17·9	28·5	26·5	17·4	9·7	20·7	20	19	22·15
23	20·3	19·6	16·8	30·5	19·0	9·3	20·7	20	19	24·89
24	22·5	21·7	21·7	22·6	21·0	8·9	20·7	20	19	21·74
25	17·3	16·6	28·1	17·4	12·1	8·5	20·7	20	19	24·38
26	19 0	22·4	28·4	19·0	12·9	8·2	20·7	20	19	19·00
27	21·0	25·2	11·7	21·0	13·8	7·8	20·7	20	19	15·97
28	23·4	23·4	14·5	23·4	14·8	7·5	20·7	20	19	17·35
29	13·8	13·8	42·5	26·5	15·9	7·3	20·7	20	19	12·89
30	14·8	14·8	8·9	16·0	17·4	7·0	20·7	20	19	13·78
31	30·6	16·0	8·9	17·4	19·0	6·8	20·7	20	19	21·74
32	19·0	17·4	9·8	19·0	21·0	6·6	20·7	20	19	24·89

TABLE SHOWING THE DIFFERENCE OF EACH SIZE FROM STUBS' GAUGE IN PERCENTAGE OF WEIGHT.

No. of Gauge.	Wynn.	South Staffordshire.	Cocker.	Walker.	American.	Briggs.	Clark.	Hughes.
000	—22·1	...	— 2·3	6	— 7·12	27·8	34·0	12·1
00	—15·2	...	0	— 2·6	— 7·8	32·5	34·1	10·8
0	— 8·6	...	0	— 2·2	— 8·7	34·1	34·1	6·0
1	2·0	1·7	0	8·5	— 7·0	39·5	38·4	0
2	— 2·8	— 5·8	— 2·8	— 1·9	—17·7	26·1	22·9	— 2·8
3	— 0·8	— 1·9	0·8	— 6·8	—21·6	22·8	18·4	0·8
4	0	— 1·7	1·7	— 3	—26·3	17·8	12·1	1·7
5	— 1·8	— 2·7	0	— 1·1	—31·6	11·6	4·9	0
6	— 2·9	4·5	— 2·9	1·0	—36·3	6·2	— 1·4	— 2·9
7	2·3	5	0	8·5	—35·7	9·4	0·3	0
8	0	3·4	— 6	8·5	—39·4	5·5	— 4·5	0
9	1·4	6·9	—10·5	11·5	—41·3	6·2	— 5·1	2·7
10	0	0	— 5.9	10·1	—42·2	5·0	— 7·3	1·5
11	0	— 5·8	0	8·5	—42·8	6·1	— 7·5	— 0
12	— 3·6	— 4·5	1·8	6·5	—46·3	4·1	—10·4	1·8
13	0	3·2	10·7	—10·8	—43·6	13·6	— 5·6	10·8
14	2·4	—15·0	17·5	11·1	—40·4	17·6	— 1·0	4·9
15	2·8	2·8	23·5	8·5	—37·2	26·7	5·2	8·5
16	0	—13·4	16·	— 7·5	—38·9	26·0	3·2	0
17	0	—13·3	7	— 5·9	—39·1	28·2	3·8	0
18	4·1	2·0	4·1	4·1	—32·8	45·4	16·3	4·1
19	4·8	— 2·4	14·8	8·5	—27·0	60·3	26·6	9·8
20	11·8	—24·1	30·6	14·8	—16·6	87·1	45·9	17·9
21	0	12·9	19·6	15·4	—20·9	81·1	39·6	12·9
22	7·2	3·6	14·8	24·5	—18·0	91·7	45·9	14·8
23	8·2	8·1	0	26·5	—18·5	94·7	46·4	8·2
24	9·3	9·3	0	29·1	—16·5	103·7	51·2	9·3
25	10·2	4·9	0	37·3	—19·9	105·1	46·4	0
26	5·6	—16·0	0	47·6	—21·6	99·7	44·6	0
27	0	— 6·2	0	61·1	—21·4	104·7	46·4	6·4
28	0	7·3	0	79·3	—18·5	116·5	53·0	14·8
29	0	—28·4	—14·8	74·8	—25·0	103·4	42·0	16·0
30	0	—23·5	—16·0	69·4	—30·1	93·4	33·2	17·4
31	21·0	0	0	97.6	—20·3	125·6	53·5	32·3
32	23·4	11·4	0	92.9	—22·0	125·3	51·6	23·5

PROPOSED STANDARD GAUGE IN MILLIMETRES.

No. of Gauge.	Diameter in Millimetres.	No. of Gauge.	Diameter in Millimetres.
000	12·50	25	·615
00	11·18	26	·550
0	10·00	27	·492
1	8·94	28	·440
2	8·00	29	·393
3	7·16	30	·352
4	6·40	31	·315
5	5·72	32	·281
6	5.12	33	·252
7	4·58	34	.225
8	4·10	35	·201
9	3·66	36	·180
10	3·28	37	·161
11	2·93	38	·144
12	2·62	39	·129
13	2·35	40	·115
14	2·10	41	·103
15	1·88	42	·092
16	1·68	43	·083
17	1·50	44	·074
18	1·34	45	·066
19	1·20	46	·059
20	1·07	47	·053
21	·960	48	·047
22	·859	49	·042
23	·768	50	·038
24	·687		

PROPOSED STANDARD GAUGE.

No. 0 = 1 Centimetre.

No. of Gauge.	Diameter in Mils.	No. of Gauge.	Diameter in Mils.
0000	550·22	24	27·05
000	492·14	25	24·20
00	440·18	26	21·64
0	393·71	27	19·36
1	352·14	28	17·32
2	314·97	29	15·49
3	281·71	30	13·85
4	251·97	31	12·39
5	225·37	32	11·08
6	201·58	33	9·91
7	180·30	34	8·86
8	161·26	35	7·93
9	144.24	36	7·09
10	129·01	37	6·34
11	115·39	38	5·67
12	103·21	39	5·07
13	92·31	40	4·54
14	82·57	41	4·06
15	73·85	42	3·63
16	66·05	43	3·25
17	59·08	44	2·90
18	52·84	45	2·60
19	47·26	46	2·32
20	42·27	47	2·08
21	37·81	48	1·86
22	33·82	49	1·66
23	30·25	50	1·49

PROPOSED STANDARD GAUGE.

No. 0 = 1 Centimetre.

No. of Gauge.	Diameter in Mils.	No. of Gauge.	Diameter in Mils.	No. of Gauge.	Diameter in Mils.	No. of Gauge.	Diameter in Mils.
000	492·13	5	225·37	12	103·21	19	47·26
¼	478·60	¼	219·17	¼	100·37	¼	45·96
½	465·43	½	213·14	½	97·61	½	44·70
¾	452·63	¾	207·28	¾	94·92	¾	43·47
00	440·18	6	201·58	13	92·31	20	42·27
¼	428·07	¼	196·03	¼	89·77	¼	41·11
½	416·30	½	190·64	½	87·30	½	39·98
¾	404·84	¾	185·40	¾	84·90	¾	38·88
0	393·71	7	180·30	14	82·57	21	37·81
¼	382·88	¼	175·34	¼	80·28	¼	36·77
½	372·35	½	170·52	½	78·08	½	35·76
¾	362·10	¾	165·82	¾	75·93	¾	34·78
1	352·14	8	161·26	15	73·85	22	33·82
¼	342·46	¼	156·83	¼	71·81	¼	32·89
½	333·04	½	152·52	½	69·84	½	31·98
¾	323·88	¾	148·32	¾	67·92	¾	31·10
2	314·97	9	144·24	16	66·05	23	30·25
¼	306·30	¼	140·27	¼	64·23	¼	29·42
½	297·88	½	136·41	½	62·46	½	28·61
¾	289·68	¾	132·65	¾	60·75	¾	27·82
3	281·71	10	129·01	17	59·08	24	27·05
¼	273·97	¼	125·46	¼	57·45	¼	26·31
½	266·43	½	122·01	½	55·88	½	25·59
¾	259·10	¾	118·65	¾	54·34	¾	24·88
4	251·97	11	115·39	18	52·84	25	24·20
¼	245·04	¼	112·22	¼	51·39	¼	23·53
½	238·30	½	109·13	½	49·97	½	22·89
¾	231·75	¾	106·13	¾	48·60	¾	22·26

APPENDIX.

HIGHFIELDS, THELWALL, NEAR WARRINGTON,
January 7, 1879.

DEAR SIR,

After reading Mr. Walker's paper and yours, which you kindly sent me, and thinking the matter over, I felt that, in the first place at least, my best course would be to explain to you shortly what I had done in the matter of the wire gauge, when I was at work on the subject.

What follows was written in consequence. If you wish it, I will answer any further questions you desire.

I have little faith in the construction of an artificial gauge. I remember Mr. J. Cocker's efforts in that line, for I talked to him often on the subject. It might do if the old gauge was a matter of feeling or prejudice only, but it is not so.

I am, yours faithfully,
(Signed) THOMAS G. RYLANDS.

LATIMER CLARK, Esq.

My attention was first called, critically, to the "Birmingham Wire Gauge" in 1851. There was then a growing demand for the knowledge of the equivalent weights and lengths of a number of the sizes.

It was very soon found, as the result of extensive enquiries, that the usual intervals and diameters were so irregular and anomalous that no reliance could be placed upon them, either from a practical or a commercial point of view. It seemed as though the "*old Birmingham Wire Gauge*" was little better than a myth.

Under these circumstances, I undertook to regulate the sizes, so as to interfere with them individually as little as possible, giving weights to each of the sets of sizes, or other information I had gained in accordance with their internal consistency. This was done somewhat after the plan of Mr. Walker, and the results are contained in the column "*Warrington," from Mr. Henley,* in your table, and, with slight changes, in the other columns which have No. 1 = 300 and No. 22 = 30.

This, my first table, was issued early in 1852.* It contained for each size, the diameter, area of section, weight of 100 yards, length of a bundle and a hundredweight, and the breaking weight in lbs. The results were obtained solely from a large series of experiments of my own. The Sp. gr.† was the outcome of much labour, in which wire rods, and wire annealed, bright, and hard drawn were distinguished, and the breaking strain constantly was found from—I should say thousands of—experiments tested in all sizes and conditions. This

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* February 28, 1852. My original MS. copy is before me.

† For Sp. gr. pieces of wire 20-30 lbs. were used, weighed in water, tested for its own Sp. gr.

table No. 1 was printed and circulated for several years: and, often with diameters slightly changed, but other columns unaltered, was freely made use of by others.*

Sometime between 1862 and 1864 I had been working at certain vital statistics, and the conditions under which I found steady curves resulting therefrom, such apparently uncertain and variable facts impressed me with the idea that the interests of capital and labour, unknown to the parties concerned, and limited by the natural properties of iron, ought to produce a steady curve for the wire gauge.

It would be tedious to detail the efforts made to decide this point. Suffice it to say, that to determine the "properties of Iron," sets of 20 pieces† were taken in the thick rod, tested for tension, strain, &c., in that state, and then drawn down according to the sizes of reputed gauges; the tests were repeated after each process of drawing, annealing, &c., in each set.

The object was to see which series of "sizes" would result in the best quality of the wire when it reached No. 22, and at each stage in its progress. Due weight being given to the results at certain points in the above experiments, and to the concurrent evidence of gauges, &c., at those points, several partial curves were obtained, and at last a complete one, extending from No. 0 to No. 22.‡

This I should have adopted as the natural and normal gauge without hesitation, had it not happened that, just as my work was completed, as if by a "particular Providence," no less than three quite independent sets of "old Birmingham sizes" came within my reach!

The youngest of these sets could be traced to 1810§: in easy going times, when, if even, the normal curve ought to be shewn.

These sets were all carefully gauged by my Holtzapffel, which I ought to have said was used throughout in these investigations, and the result was the Table No. 2, which you call *R. Bros, May*, 1866, and which I confidently left as my ultimatum.

It is not strictly in accordance with the normal or natural curve I had obtained, because in the process of manufacture wire is not annealed at every size, and the influence of this upon the gauge, though slight, became very perceptible.

It is not strictly in accordance with any one of the old sets of sizes, because they were not quite consistent among themselves, but the differences were slight, and in cases where there was any doubt, I favoured the "normal curve." I may say, however, that in no case did the doubt amount to ·003 inch.

One other point should be mentioned—the fractions of an inch $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{10}$, $\frac{1}{16}$, and $\frac{1}{32}$, were not *exact* in the decimals. The table was intended to meet commercial rather than scientific wants; the differences, however, are trifling, the

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* I have seen no such table hitherto, which is not based upon my own data.

† Separate sets for different "makes" of iron were also tested throughout.

‡ *I mentioned this* and what follows below, after the reading of your paper at Exeter.

§ *Not less than 50 years* old.

decimal, I think, *always* a shade thicker for working purposes. Unfortunately, my original calculations, &c., are lost; they were neglected when I considered the work done, and found the results so generally adopted.*

Lastly, in reply to your questions:—The "most arithmetic gauge," until evidence to the contrary is produced, is, I believe, the one I have attempted to explain. I say this because I know of no other one which is based upon such an amount of independent testimony, and satisfies so well the conditions, both technical and scientific. I have now no interest in the matter; it has been an effort even to write what I have done, therefore I should cheerfully adopt any of the suggested gauges if I could see that they offered any advantages in the wire mill, and out.

As an "International Gauge," so far as my experience and recollection go, the Millimetric Diameters† were generally adopted abroad; the specifications did, however, sometimes give the diameter in round millimetres; this practically was of no importance, the effect was nil.

From the remarks in your second paper, I hope that ordinarily drawn steel wire need not affect the sizes, but this is only an opinion.

---

* You will see that I soon lost faith, if I ever had any, in *theoretical* gauges! Quality first, and then cost of production, lie at the root of this matter.

† Introduced into the Tables for the purpose.

ORIGINAL COMMUNICATIONS.

YEZD, *8th November*, 1879.

The Secretary,

Society of Telegraph Engineers, London,

SIR,

For your statistical notes I beg to inform you that we have this year added three lines to our net of telegraphs. They are—

1st. Resht-Enzelí, 13 miles, with Enzelí as a new station.

2nd. Semnán-Fírúzkúh, 44 miles, with Fírúzkúh as a new station, and

3rd. Ispahan-Yezd-Kermán, 396 miles, with the following new stations:—Qohpáyeh, Náin, Agdá, Yezd, Kermánsháhán, Enár, Bahrámábád, Kermán.

The total length of Persian worked telegraph lines is now 2,096 miles. They have all one wire only. Including the 1,150 miles of line, with 3,450 miles of wire worked by the English Government and the Indo-European Company, the total length of lines in Persia is 3,246 miles, the length of wire 5,546 miles. The number of Persian telegraph stations is now 65. The total number of telegraph stations in Persia is 71; 20 of these are English telegraph stations, and 14 of the latter are also Persian stations, the remaining 6 being small controlling stations.

The wire for the new lines was Messrs. Johnson's No. 9 B.W.G.; the supports are of wood, mostly poplar. The instruments were from Siemens at Berlin.

I am, Sir,

Yours faithfully,

A. HOUTUM-SCHINDLER,

Inspector General of Persian Telegraphs.

THE EXCHANGE TELEGRAPH COMPANY, LIMITED,
7 AND 8, CORNHILL, LONDON, E.C.,
2nd December, 1879.

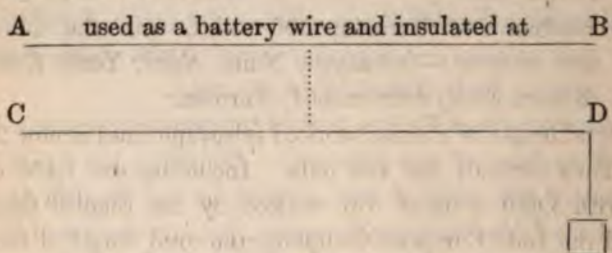
To the Secretary,
Society of Telegraph Engineers.

SIR,

I see in a report of the proceedings at the Meeting of the Society on the 26th ultimo, that a paper was read on a method of localising a contact between telegraph wires.

In the second edition of Culley's "Handbook of Telegraphy," published some years ago, will be found a far simpler method which I devised for the same purpose, and with which I have succeeded frequently in indicating the exact locality of a fault on the Ex. Tel. Co.'s lines.

The plan is as follows:—



The two portions of the line C D on either side of the fault are made equal by the addition of resistance to the shorter end, the balance being obtained either by means of a Wheatstone bridge or differential galvanometer and resistances.

The recorded resistance of C D, or its resistance with A B insulated, is next taken, when the following is obtained:—

$$\frac{\text{Res. of C D} \pm \text{Res. required to balance}}{2} = \text{resistance of line}$$

C D from testing place to the fault.

This method has the advantage of being independent of variations in the resistance of the fault during testing.

I am, Sir,
Yours obediently,
F. HIGGINS.

F. H. WEBB, Esq.

STATEMENT OF RECEIPTS AND EXPENDITURE FOR THE YEAR 1879.

RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance, December 31st, 1878				204	14	5
„ Subscriptions paid up for previous years ...				324	16	0
„ Subscriptions for 1879				887	16	6
„ Subscriptions paid in advance				32	12	6
„ Entrance Fees				59	17	0
„ Life Compositions...				72	0	0
„ Publishing Fund				6	5	0
„ Sale of Journals	115	18	9			
„ Advertisements in Journal	12	12	0			
				128	10	9
„ Subscriptions for Ronalds' Catalogue				6	10	0
„ Special Loan from Members of the Council	675	0	0			
Less since paid off... ...	225	0	0			
				450	0	0
„ Premium Fund, Mr. Fahie's donation ...				100	0	0
„ Do. Interest thereon...				5	0	0
				£2,278	2	2

EXPENDITURE.

	£	s.	d.	£	s.	d.
By Liabilities 1878, paid off				921	15	0
„ Salaries and Clerical Assistance ...	194	13	6			
„ Shorthand Reporter	19	8	6			
„ Attendance and Refreshments at Meetings	19	13	2			
„ Printing, Lithography, and Stationery, and advertising the Journal ...	322	5	6			
„ Ronald's Library, preparing Catalogue and Insurance	54	19	8			
„ „ Printing Catalogue, on account	102	18	6			
„ Rent, Gas, and Firing	162	6	6			
„ Postage, Cost of forwarding Journals, and petty Office Expenses	98	18	0			
„ Expenses of Local Hon. Secretaries ...	5	5	9			
„ Loss in Exchange and charges on Foreign Drafts	0	14	2			
„ Interest	30	6	3			
				1,011	9	6
„ Balance in hands of Bankers, of which £100 (Mr. Fahie's donation) has to be invested, and £5 stands to the credit of Premium Fund (Interest Account), leaving £234 4s. 5d. available for current Expenditure ...	339	4	5			
„ Balance in hands of Secretary ...	5	13	3			
				344	17	8
				£2,278	2	2

We have examined the above Account with the Vouchers and Books of the Society, and find it to be correct, leaving in the hands of the Bankers Three Hundred and Forty-Four Pounds, Seventeen Shillings and Eight Pence on the 31st of December, 1879.

J. WAGSTAFF BLUNDELL,
FRED. CHAS. DANVERS.

CONFERENCE RECEPTION FUND ACCOUNT.

	£	s.	d.		£	s.	d.
To Subscriptions received	638	19	6	By Refreshments, Cost of Fitting up Refreshment Rooms and Lighting	302	3	0
				„ Band	37	10	0
				„ Attendance	23	19	11
				„ Floral decorations	3	2	9
				„ Electric light...	19	13	3
				„ Gas	5	16	11
				„ Expenses in connection with Exhibits	7	1	0
				„ Printing Catalogue, Cards, and Circulars ...	20	6	6
				„ Secretary	25	0	0
				„ Postages and Petty Expenses	22	0	0
					466	13	4
				„ Balance to be appropriated by consent of the Subscribers, to the binding of the Library ...	172	6	2
	£638	19	6		£638	19	6
To Balance brought down	172	6	2	By Amount paid on account of binding	17	16	5
				„ Clerical Assistance to Librarian in preparing Books, &c., for binding	3	7	8
				„ Balance in the hands of Bankers...	151	2	1
	£172	6	2		£172	6	2

We have examined the above Account with the Vouchers and Cash Books, and find it to be correct, leaving in the hands of the Bankers One Hundred and Fifty-One Pounds, Two Shillings and One Penny.

J. WAGSTAFF BLUNDELL,
FRED. CHAS. DANVERS.

ESTIMATE OF ASSETS AND LIABILITIES ON THE 31ST DECEMBER, 1879.

ASSETS.	£	s.	d.	£	s.	d.
Balance in the hands of Bankers ...	339	4	5			
" " " Secretary ...	5	13	3			
				344	17	8
Unpaid Subscriptions				368	0	0
Estimated value of furniture, viz., as per last Balance Sheet	200	0	0			
Less depreciation	20	0	0			
				180	0	0
Objects presented to the Society (valued at £225, but this sum is not commercially realisable)						
Journals in stock, estimated realisable value ...				642	0	0
				£1,534	17	8

LIABILITIES.	£	s.	d.
Balance of Special Loan, as per Balance Sheet ...	450	0	0
Subscriptions received in advance	32	12	6
Mr. Fahie's Donation to be invested...	100	0	0
Interest thereon (Premium Fund)	5	0	0
(There are no other Liabilities)			
Balance *Cr.*	947	5	2
	£1,534	17	8

E. GRAVES, *Honorary Treasurer.*
F. H. WEBB, *Secretary.*

INDEX.

Vol. VIII.

514a

THE SOCIETY

OF

TELEGRAPH ENGINEERS.

ESTABLISHED 1871.

LIST OF OFFICERS AND MEMBERS.

(CORRECTED TO JANUARY 31, 1879.)

NOTICE.

It is particularly requested that any change of Residence or Address may be communicated at once to the Secretary.

The Society of Telegraph Engineers.

COUNCIL.—1879.

President.

LIEUT.-COL. J. U. BATEMAN-CHAMPAIN, R.E.

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ST. GEORGE LANE FOX.
CAPTAIN M. T. SALE, R.E.

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Auditors.

J. WAGSTAFF BLUNDELL, 12, Delahay Street, Westminster, S.W.
FREDERICK C. DANVERS (India Office).

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Hon. Solicitors.

Messrs. WILSON, BRISTOWE & CARPMAEL, 1, Copthall Buildings, E.C.

Chairman of the Editing Committee.—PROFESSOR W. E. AYRTON.

Secretary.—F. H. WEBB.

Bankers.

Messrs. COCKS, BIDDULPH & Co., 43, Charing Cross, S.W.

Library and Offices of the Society.

4, Broad Sanctuary, Westminster, S.W.

LOCAL HONORARY SECRETARIES AND TREASURERS.

James Allen, Telegraph Superintendent Great Southern Railway, Buenos Ayres	ARGENTINE REPUBLIC.
Don José Aparicio, Calle de Valgame Dios, 2 pral., Madrid	SPAIN.
John Aylmer, Civil Engineer, 4, Rue de Naples, Paris	FRANCE.
C. Burton, Telegraph Engineer, Buenos Ayres	REPUBLIC OF BOLIVIA.
J. M. Collette, Engineer of the Netherlands Telegraphs, The Hague	The NETHERLANDS.
E. C. Cracknell, Superintendent of the New South Wales Telegraphs, Sydney	NEW SOUTH WALES.
James Dakers, Manager, Montreal Telegraph Company, Montreal	CANADA.
Le Commandeur E. D'Amico, Director-General of the Italian Telegraphs, Rome	ITALY.
Frédéric Delarge, Engineer of the Belgian Telegraphs, Brussels	BELGIUM.
Stephen D. Field, California Electrical Works, 134, Sutter Street, San Francisco	WESTERN AMERICA.
Andrew Jamieson, C.E., Eastern Telegraph Company, Malta	MALTA AND THE MEDITERRANEAN.
C. L. Madsen, Great Northern Telegraph Company, 41, Gamle Kongevei, Copenhagen	DENMARK.
C. Nielsen, Director-General of the Norwegian Telegraphs, Christiania	NORWAY.
J. Perry, B.E., Professor of Engineering, Imperial College, Tokei, Japan	JAPAN.
J. R. Preece, Indo-European Government Telegraphs, Shiraz	PERSIA.
Dr. Werner Siemens, Berlin	GERMANY.
F. G. Teale (Acting Local Hon. Sec. and Treasr.), Indian Government Telegraphs, Calcutta	INDIA.
G. G. Ward, Gen. Superintendent of the Direct United States Cable Company, New York	NORTH AMERICA.

Society of Telegraph Engineers.

LIST OF MEMBERS,

CORRECTED UP TO JANUARY, 1879.

† *Members who have served as Vice-Presidents.*

* *Members who have served on the Council.*

HONORARY MEMBERS.

Sir George Biddel Airy, K.C.B., D.C.L., LL.D., *Astronomer Royal.*	Royal Observatory, Greenwich.
Gen. Sir Edward Sabine, R.A., K.C.B., D.C.L., LL.D.	13, Ashley Place, Westminster, S.W.
Professor Wilhelm Weber, F.R.S.	Göttingen.
FRANK IVES SCUDAMORE, C.B. . *(Past President.)*	British Post Office, Constantinople.
Sir William Fothergill Cooke .	Farnham, Surrey.

Total Number of Honorary Members . . 5.

FOREIGN MEMBERS.

Ailhaud, M.	Inspector-General of the French Telegraphs, Paris.
Akrell, Major Axel . . .	Royal Swedish Telegraphs, Stockholm, Sweden.
Alteneck, F. Von Hefner . .	94, Markgrafen Strasse, Berlin.
APARICIO, DON JOSÉ . . *(Honorary Secretary for Spain.)*	Calle de Valgame Dios, 2 pral., Madrid.
Arendrup, Capt. C. H. . . .	Royal Danish Engineers, Copenhagen.
Arisz, L.	Divisional Inspector of Netherlands Telegraphs, Utrecht.
Atkins, George W. E. . . .	Western Union Telegraph Co., 197, Broadway, New York.

BACHELOR, CHARLES . . .	Menlo Park, New Jersey, U.S.
BARON, H.	103, Rue de Grenelle, St. Germains, Paris.
BEJAR, Don ALEJANDRO, DE . .	Sub-Director of Spanish Telegraphs, Carthagena, Spain.
BLAVIER, EDOUARD . . .	103, Rue de Grenelle, St. Germains, Paris.
BOLTON, MARCO ANTONIO . .	Cia del Telegrafo, Trasandino, Valparaiso, Chili.
BONTEMPS, CHARLES . . .	103, Rue de Grenelle, St. Germains, Paris.
BOOMSMA, A.	The Netherlands State Telegraph Office, Driebergen, Holland.
BOURNE	The Montreal Telegraphs, Canada.
BOYLE, R. K.	73, Beekman Street, New York, U.S.
BRÉGUET, M., Senr. . . .	39, Quai de l'Horloge, Paris.
BRIEVE, C.	Divisional Inspector of Netherlands Telegraphs, Roosendaal.
BROOKS, DAVID	22, South Twenty-first Street, Philadelphia, U.S.
BURTON, CHARLES, A.I.C.E. . *(Honorary Secretary for the Republic of Bolivia.)*	Telegraph Engineer, Buenos Ayres.
CABRERA, FERNANDO . . .	State Telegraph Administration, Santiago, Chili.
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CARÈME, FELIX	103, Rue de Grenelle, Paris.
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CARSTENS, P. BOGILAUS . . .	Royal Danish Telegraphs, Ronne, Bornholm, Sweden.
COLLETTE, J. M. . . . *(Honorary Secretary for the Netherlands.)*	Engineer of the Netherlands Telegraphs, The Hague.
CONCHARD-VERMEIL, EMILE GEORGES	French Government Telegraphs, Marseilles.
COROMIÑA, Don VICENTE . .	Director of Spanish Telegraphs, Madrid.
COURTENAY, L. W. . . .	45, Rue de Luxembourg, Paris.
D'AMICO, M. le Commandeur, E. . *(Honorary Secretary for Italy.)*	Director-General of the Italian Telegraphs, Rome.
DEHMS, Dr. FR.	Imperial Telegraphs, Constanz, Baden.

DELARGE, FRÉDÉRIC . . (*Honorary Secretary for Belgium.*)	Engineer of the Belgian Telegraphs, Brussels.
DELAFIELD, A. FLOYD . . .	Norolon, Connecticut, U.S.
D'INFREVILLE, GEORGE . . .	129, Broad Street, New York.
DREYER, Lieut. FREDERICK . .	Royal Danish Navy, Shanghai.
DUCOTÉ, J.	103, Rue de Grenelle, St. Germains, Paris.
DWIGHT, H. P.	General Superintendent, Montreal Telegraph Company, Toronto.
ECKERT, Gen. THOMAS T. . .	President, Atlantic and Pacific Telegraph Company, 145, Broadway, New York, U.S.
EDISON, THOMAS A. . . .	Menlo Park, New Jersey, U.S.
ELDIK, R. K. VAN . . .	Divisional Inspector of Netherlands Telegraphs, Venlo, The Netherlands.
EVERS, I. C.	Inspector of Railway Telegraphs, Utrecht, The Netherlands.
FARMER, Professor MOSES G. . .	U.S. Torpedo Station, Newport, R.I.
FORSMANN, C. M.	Swedish Government Telegraph Administration.
FRISCHEN, CARL	94, Markgrafen Strasse, Berlin.
FRÖHLICH, Dr. OSCAR . . .	94, Markgrafen Strasse, Berlin.
GALANTE, Don JOSÉ . . .	Inspector of Spanish Telegraphs, Government Department, Seville.
GARAY, Don FELIX . . .	Director of Spanish Government Telegraphs, Vitoria.
GARCIA DEL REAL, Don FEDERICO .	Director of Spanish Telegraphs, Madrid.
GIMENEZ, Don RAFAEL . . .	Santiago, Chili.
GIRARDIN, JOSEPH . . .	Director of Belgian Telegraphs, Brussels.
GRACE, G. H.	Western Union Telegraph Company, 197, Broadway, New York, U.S.
GRAMACCINI, JULES H. F. . .	Government Telegraph Department, Marseilles.
GRAY, ELISHA	220, Kinzie Street, Chicago, Illinois, U.S.
GUILLEAUME, F. C. . . .	Cologne, Rhenish Prussia.

HAMILTON, GEORGE A. . . .	Western Union Telegraph Company, 197, Broadway, New York, U.S.
HANSEN, EMIL A. . . .	Fredericia, Denmark.
HAUFF, P. P.	Atlantic and Pacific Telegraph Company, 145, Broadway, New York, U.S.
HELLAND, GEORGE T. . . .	Great Northern Telegraph Company, Shanghai.
HENNINGSEN, JACOB . . .	Great Northern Telegraph Company, Foochow, China.
HEQUET, A.	103, Rue de Grenelle, St. Germains, Paris.
HONCKE, Captain HEINRICH . .	Danish Royal Engineers, Copenhagen.
HOPSTOCK, J. S. K. . . .	Christiania, Norway.
HOSKLÆR, Captain V. . . .	Royal Danish Engineers, Copenhagen.
HOSSLIN, E. Von	Bavarian Telegraphs, Munich.
HUGHES, Professor, D. E. . .	Submarine Telegraph Company, Threadneedle Street, E.C.
IBARROLA, Don JUAN MARTIN . .	Director of Spanish Telegraphs, Madrid.
ITURRIAGO, Don ENRIQUE . .	Director of Spanish Telegraphs, Madrid.
JABLOCHKOFF, PAUL . . .	31, Rue d'Offernont, Paris.
JAGER, T. DE	Divisional Inspectors of Netherlands Telegraphs, Leeuwarden, The Netherlands.
JONES, L. BIGELOW . . .	Boston and Albany Railway Company, Allston, Mass., U.S.
KELLOGG, MILO G. . . .	220, East Kinzie Street, Chicago, U.S.
LACOINE, EMILE	Director of Telegraphs, Constantinople.
LADD, GEORGE J. . . .	Western Union Telegraph Company, San Francisco, U.S.
LAGARDE, JOSEPH . . .	103, Rue de Grenelle, St. Germains, Paris.
LANCKERT, EDWARD . . .	94, Markgrafen Strasse, Berlin.
LARKIN, THOMAS J. . . .	Imperial Government Telegraphs, Japan.
LASARD, Dr. ADOLPHE . . .	German Union Telegraph Company, Werder Strasse, Berlin.

LOBIT, Don LUIS . . .	Sub-Director of Spanish Telegraphs, Madrid.
LOPEZ DE OCHOA, Don ANTONIO	Inspector-General of Spanish Telegraphs, Madrid.
LORENTZEN, VALDEMAR . .	Royal Danish Telegraphs, Copenhagen.
LÜDERS, His Excellency Gen. Von	Director-General of the Imperial Russian Telegraphs, St. Petersburg.

MADSEN, C. L. . . . *(Honorary Secretary for Denmark.)*	41, Gamle Kongevei, Copenhagen.
MARTIAL, A. DE SAINT . .	Secretary of the International Bureau, Berne.
MAYNARD, G. C. . . .	1423, G. Street, Washington, U.S.
MEYER, B.	No. 1, Boulevard St. Denis, Paris.
MONCEL, Count THEODORE DU .	7, Rue de Hambourg, Paris.
MOOLENAAR-REEDER, H. A. .	Divisional Inspector of Netherlands Telegraphs, Amsterdam.
MOORE, GEORGE W. . .	Erie, Pennsylvania, U.S.
MORA, Don FRANCISCO . .	Inspector of Spanish Telegraphs, Government Department, Madrid.
MORA, Don ORESTES DE . .	Director of Spanish Telegraphs, Government Department, Barcelona.
MULVAD, CARL . . .	Royal Danish Telegraphs, Copenhagen.

NAGEL, J. H. . . .	Netherlands State Telegraph Office, Arnhem.
NASVARRA, W. S. . . .	Telegraph Department, Teheran, Persia.
NIAUDET, ALFRED . .	6, Rue de Seine, Paris.
NIELSEN, C. . . . *(Honorary Secretary for Norway.)*	Director-General of the Norwegian Telegraphs, Christiania.
NIELSEN, C. P. . . .	Great Northern Telegraph Company, Amoy, China.
NIELSEN, F. C. C. . .	Great Northern Telegraph Company, Hong-Kong, China.
NIELSEN, JULIUS . . .	Great Northern Telegraph Company, Copenhagen.
NIELSON, HUGH . . .	Telegraph Engineer, Toronto, Canada.

ORDUNA, Don EMILIO . .	Sub-Director of Spanish Telegraphs, Madrid.

Paulsen, Nielo M.	Great Northern Telegraph Company, Liban, Russia.
Pellegrino, Commander Herico	Italian Telegraphs, Florence.
Pentasuglia, Commander G.B.	Italian Telegraphs, Florence.
Petersen, Carl A.	Royal Danish Telegraphs, Fredericia, Denmark.
Petersen, Julius	Great Northern Telegraph Company, Paris.
Petersen, J. A. P.	Great Northern Telegraph Company, Godby, Aland.
Pforden, Frederick von des	Eastern Extension Telegraph Company, Batavia.
Pope, Frank L.	Elizabeth, New Jersey, U.S.
Prados, Don Julian Alonzo	Director of Spanish Telegraphs, Madrid.
Prescott, George B.	Western Union Telegraph Company, 197, Broadway, New York, U.S.

Rau, Edouard	75, Rue d'Arlon, Brussels.
Raymond, Mons.	Director of Stores and Works, 103, Rue de Grenelle, St. Germains, Paris.
Raynaud, Jules	103, Rue de Grenelle, St. Germains, Paris.
Redonet, Don José de	Director of Spanish Telegraphs, Government Department, Santander, Spain.
Rentzch, Fred. John	Eastern Extension Telegraph Company, Banjoewargie, Java.
Repsold, J. G.	25, Alsterweg, St. George, Hamburg.
Rothen, Timotheus	Engineer-in-Chief of the Swiss Telegraphs.
Rousseau, A. J.	80, Nevsky, St. Petersburg.
Russell, Thorwold F.	Great Northern Telegraph Company, Nagasaki, Japan.

Santman, L. J.	Inspector of Dutch-Indian Government Telegraphs, Batavia.
Sargent, W. D.	3708, Baring Street, West Philadelphia, Pennsylvania, U.S.
Salvatori, F.	Italian Telegraphs, Rome.
Schonheyder, Lieut. Sophus	Royal Danish Telegraphs, Nykjöbing-Paa-Falster, Denmark.
Schultz, C. A.	Great Northern Telegraph Company, Shanghai.
Severn, Henry A.	Herne Hill, London.

SIEMENS, Dr. WERNER	94, Markgrafen Strasse, Berlin.
SMITH, GERRITT	Western Union Telegraph Company, 197, Broadway, New York, U.S.
SOLDAN, Don CARLOS PAZ	Lima, Peru.
SONNE, CARL C.	Great Northern Telegraph Company, Shanghai.
SPANG, H. W.	Reading, Pennsylvania, U.S.
STARING, WILLIAM	Director of the Netherlands Telegraphs, The Hague.
STEARNS, J. B.	Care of J. S. Morgan & Co., 22, Old Broad Street, E.C.
STRAUBE, OTTO	Sub-Director of Telegraphs, No. 187, Calle Cangallo, Buenos Ayres.
SUENSON, ALBERT	Great Northern Telegraph Company, Hong-Kong, China.
SUENSON, Capt. EDWARD	Great Northern Telegraph Company, Copenhagen.
SUMMERS, C. H.	Western Union Telegraph Company, Chicago, Illinois, U.S.
SWINYARD, THOMAS	Managing Director, Dominion Telegraph Company, Toronto, Canada.
TARRAT, Don NARCISO	Sub-Director of Spanish Telegraphs, Madrid.
THOMSEN, VALDEMAR	Great Northern Telegraph Company, Nystad-in-Finland, Russia.
TOBLER, Dr. ADOLPH	Federal Polytechnic School, Zürich.
TRANT, LAWRENCE B.	Provincial Telegraph Department, 103, Calle Cangallo, Buenos Ayres.
TRANT, PETER NICHOLAS	Provincial Telegraph Department, Rosario, Argentine Republic.
UCELAY, Don PASCUAL	Director of Spanish Telegraphs, Zaragoza.
VAN HOEVENBERG, HENRY	Atlantic and Pacific Telegraph Company, 145, Broadway, New York.
VASQUEZ, Don FRANCISCO	Sub-Director of Spanish Telegraphs, Madrid.
VIALE, Chevalier CLEMENTE	Italian Telegraphs, Florence.

VIANISI, Marquis L. . . .	Messina.
VIGIL, Don PRIMITIVO . . .	Chef de Station, Spanish Telegraphs, Madrid.
VINCHENT, JULIEN . . .	Inspector-General of the Belgian Public Works, Brussels.
VOGEL, CARL	94, Markgrafen Strasse, Berlin.
WALL, H. C. T. Van de . . .	Inspector of Dutch-Indian Government Telegraphs, Batavia.
WISSE, W. J.	Sub-Director of the Government Telegraph Office, The Hague.
ZETZSCHE, Professor Dr. K. E. . .	Professor of Telegraphy, Royal Saxon Polytechnicon, Dresden.

Total Number of Foreign Members . . 153

MEMBERS.

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ADAMS, R. W. P.	Indian Government Telegraphs.
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SMITH, BENJAMIN . . . . Eastern Telegraph Company, Alexandria.

SMITH, FREDEICK . . . . Caledonia Iron Works, Halifax, Yorkshire.

SMITH, JAMES . . . . West India and Panama Telegraph Company, Kingston, Jamaica.

SMITH, JOSEPH H. . . . Post Office, Selby.

SMITH, JOSEPH STANLEY, C.E. . . Westmount, Grange Road, Shanklin, Isle of Wight.

SMITH, A. TOULMIN . . . Selbourne Chambers, Chancery Lane.

SMYTHE, JOHN . . . . Valentia, Ireland.

SPAGNOLETTI, HYLTON . . . 4, Circus Street, Marylebone, W.

SPRATT, G. O. . . . . Eastern Telegraph Company, Porthcurno, Penzance.

SQUIRES, FREDERICK . . . Anglo-American Telegraph Company Brest.

SQUIRES, GEORGE . . . . Anglo-American Telegraph Company St. Pierre, Newfoundland.

STACEY, CHARLES . . . . Eastern Telegraph Company, Aden.

STACEY, GEORGE B. . . . Eastern Telegraph Company, Bombay.

STAMFORD, W.	Government Telegraphs, King William's Town, Cape Colony.
STARKE, HENRY	Government Telegraph Department, Brisbane, Queensland.
STEET, G. C., F.R.C.S. . . .	130, King Henry's Road, Hampstead.
STELGES, J.	South Eastern Railway Telegraph Works, Tunbridge.
STEVENS FREDERICK P. . . .	Port Darwen, South Australia.
STEVENS, HENRY	Port Darwen, South Australia.
STEVENSON, ED. ALF. . . .	Telegraph Construction and Maintenance Company, Enderby's Wharf, East Greenwich, S.E.
STEVENSON, GEORGE . . .	Eastern Telegraph Company, Alexandria, Egypt.
STEWART, D.	Postal Telegraphs, Glasgow.
STOKES, CHARLES S. . . .	7, North Terrace, Brompton, S.W.
STOKES, HENRY L. S. . . .	7, North Terrace, Alexandra Square, S.W.
STONE, WILLIAM H. . . .	Japanese Government Telegraphs, Tokio.
STOTT, JAMES	353, Bridgman Street, Bolton.
STOUT, ROBERT	Postmaster, Lerwick, Shetland Isles.
STRAUBE, ALBERT A. L. . . .	12, Queen Anne's Gate, Westminster, S.W.
STUART, J. D. B.	Direct United States Cable Company, 16, Broad Street, New York.
SUDWORTH, S.	
SULLIVAN, HERBERT W. . . .	Eastern Telegraph Company, Malta.
SWEMMER, B.	Government Telegraphs, King William Town, Cape of Good Hope.
SYMINGTON, ROBERT STEVENSON .	Telegraph Engineer, Glasgow.

TALBOT, JAMES E. . . .	The Post Office, Margate.
TANSLEY, WILLIAM . . .	Postal Telegraphs, Portarlington.
TAPP, GEORGE R.	18, Queen Margaret's Grove, Mildmay Park, N.
TAYLOR, FRANK A. . . .	Eastern Telegraph Company, Lisbon.
TAYLOR, WILLIAM GRIGOR . .	Eastern Extension Telegraph Company, Sydney.
TEALE, WALTER	Imperial Government Telegraphs, Tokio, Japan.
THWAITES, JAMES . . .	25, St. John's Street Road, Clerkenwell, E.C.
THEILER, RICHARD . . .	86, Canonbury Road, N.

Thomas, G.	Brazilian Submarine Telegraph Company, Pernambuco.
Thompson, F. O.	Direct United States Cable Company, Palmerston Buildings, E.C.
Thompson, Thomas	Post Office Telegraphs, Ramsgate.
Thornton, F.	Inspector of Telegraphs, Fauresmith, Orange Free State, South Africa.
Tiddy, William N.	4, George Street, Hanover Square, W.
Tilly, G. B.	Eastern Telegraph Company, Gibraltar.
Tisley, S. C.	172, Brompton Road, S.W.
Tonking, Richard	2, Calver's Road, East Greenwich, S.E.
Topping, F. W.	Direct United States Cable Company, Ballinskelligs.
Tranfield, F. T.	Anglo-American Telegraph Company, Valentia.
Trippe, C.	Anglo-American Telegraph Company, Heart's Content.
Trott, J. G.	13, Dartmouth Terrace, Lewisham, S.E.
Truman, Charles	23, Old Burlington Street, W.
Tubb, Albert	Postal Telegraphs, Southampton.
Tuck, W.	Eastern Telegraph Company, Suez, Egypt.
Tuffield, T. S.	Agencia de l'Empresa, Telegrafo Submarine, Peru.
Tunbridge, W. T.	London and North Western Railway, Stafford.
Turner, J. H.	Postal Telegraphs, Cambridge.
Turner, W., Sergt.-Major R.E.	Postal Telegraphs, Canterbury.
Tyler, W. J.	106, Cannon Street, E.C.

Uren, John George	Penzance.

Venndt, C. F.	Great Northern Telegraph Company, London.
Vere de Vere, Arthur	Eastern Telegraph Company, Constantinople.
Verney, Captain R.N.	Rhianva, Bangor, North Wales.
Volk, Magnus	51, Preston Street, Brighton.
Vyle, Samuel	Postal Telegraphs, Middlesboro'.
Vyle, W. W.	Postal Telegraphs, Manchester.

WALKER, WILLIAM K.	Rangoon.
WALROND, T. C. T.	5, Netherwood Road, West Kensington Park, W.
WALTON, JOHN	Postal Telegraphs, Birmingham.
WARD, H. R. P.	1, Basingbourne Villas, Albion Road, Tunbridge Wells.
WARNER, R. A.	Cassilla, No. 777, Buenos Ayres.
WARREN, J. D.	19, Pelham Street, South Kensington.
WARREN, WILLIAM	George Town, Tasmania.
WATERS, HERBERT M.	9, Park Terrace, Greenwich, S.E.
WATKIN, Capt. R.A.	6, Lansdowne Place, Citadel Road, Plymouth.
* WATSON, C. M., Lieut. R.E.	War Office, Whitehall.
WATT, GEORGE W. M.	
WEATHERALL, T. E.	Telegraph Construction and Maintenance Company, Greenwich.
WEBB, F. H. *(Secretary.)*	9, Bernard Street, Regent's Park Road, N.W.
WEBB, E. M.	62, Warwick Gardens, Kensington, W.
WEBBER, T. B.	Telegraph Department, Great Western Railway Company, Plymouth.
WEBSTER, J. K.	Anglo-American Telegraph Company, Brest.
WEEDON, E.	Anglo-American Telegraph Company, Heart's Content.
WELLS, W. LEWIS	Submarine Cables Trust, 66, Old Broad Street, E.C.
WERDERMANN, RICHARD	4, Prince's Street, Stamford Street, S.E.
WEST, GEORGE	
WESTON, ROBERT W. GIBBS	Bervie House, Thurloe Place, Lower Norwood.
WHITE, F. H.	Anglo-American Telegraph Company, St. Pierre.
WHITMORE, MORTIMER, Lieut. R.E.	Postal Telegraphs, Bristol.
WIGAN, GORDON	2, Brick Court, Temple, E.C.
WILDE, EDWIN	Postal Telegraphs, Doncaster.
WILKINSON, HENRY D.	Eastern Extension Telegraph Company, Singapore.
WILLIAMS, A. G.	Anglo-American Telegraph Company, Heart's Content.
WILLIAMS, J.	Indo-European Government Telegraphs, Persia.
WILLMOT, JOSEPH	Postal Telegraphs, General Post Office, E.C.
WILMOT, T. J.	Direct United States Cable Company, Rye Beach, U.S.

Wilson, C. H.	Eastern Extension Telegraph Company, Hong Kong.
Windeler, Henry . . .	Anglo-American Telegraph Company, Valentia, Ireland.
Wood, Major Alexander . .	Abbey Wood, Kent.
Woodcock, W. . . .	Anglo-American Telegraph Company, Heart's Content.
Woods, James W. . . .	Engineering Department Postal Telegraphs, Oxford.
Woollen, C. H. . . .	Postal Telegraphs, Exeter.
Wray, Leonard . . .	Woodend House, Walthamstow.
Wright, James . . .	Messrs. Siemens Bros., 12, Queen Anne's Gate, S.W.

Yeates, Horatio . . .	33, King Street, Covent Garden.

Total Number of Associates . . . 472

STUDENTS.

Andrews, J. D. F.	Messrs. Siemens Bros., Charlton, S.E
Barclay, L. D.	33, Culford Road, Downham Road, Dalston, N.
Boyes, J.	Eastern Telegraph Company, 66, Old Broad Street, E.C.
Davies, George L.	Eastern Telegraph Company, Singapore.
Gee, Basil	22, Cambridge Street, Hyde Park.
Grindle, George A.	Linden House, Cambridge Gardens, Notting Hill, W.
Kennelly, A. E.	Eastern Telegraph Company, Malta.
Kirkman, John P.	4, Thurlow Road, Hampstead.
Lewis, Robert	Canada Villa, Cambridge Road, Anerley, Surrey.
Mehrtens, John	20, Christian Street, Commercial Road, E.
St. George, A. F.	India Rubber Company, 106, Cannon Street, E.C.
Weaver, A. C. M.	9, Clifton Villas, Camden Town, N.W.

Total Number of Students 12

TOTAL NUMBER OF MEMBERS.

Honorary Members	5
Foreign Members	153
Members	332
Associates	472
Students	12
Total	974

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